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VOL. XXI. No. 1 January, 1914

THE PSYCHOLOGICAL REVIEW

INDIVIDUAL DIFFERENCES BEFORE, DURING AND AFTER PRACTICE

BY H. L. HOLLINGWORTH
Columbia University

The literature of mental tests may be said to fall under two general headings, according as the purpose of the investigation reported has been (1) the immediate practical use of tests as instruments in the solution of some ulterior problem, or (2) the critical examination, analysis and testing of the instruments themselves. This paper belongs to the second of these groups. In the direct application of mental tests it has been too often assumed that the actual performance of an individual, in one or a dozen trials at a given task, is, in some way or other, significant of that individual's final capacity for such work. It is true that several investigators (notably Whitley and Wells) have studied the effects of practice on individual differences. These workers were interested above all in questions as to relative rate of improvement, or amount or permanence of gain. Such studies have produced suggestive results, although they have been based, for the most part, on records of only a few subjects or on a relatively small number of practice trials.

Thus Whitley concludes, " . . . the criticism that a single trial is unreliable is true but need not be exaggerated, since other facts . . . also enter in to make trials unreliable. To overcome this at least two trials should be made of any test, preferably in addition to fore-exercise in similar work. . . . The criticism that giving only a few trials measures not the mental process supposedly tested but merely adaptability to

strange conditions . . . is seldom of weight. . . . The criticism that tests measure the degree or amount of previous similar experience rather than actual capacity is true not only of such tests but of any form of mental measurement. It should operate only against expecting too much of the tests, not against their use, but rather, in fact, of repeating them at stated intervals. The only alternative,—testing subjects with no similar previous experience or else those whose training had brought them to the physiological limit—would be impracticable and out of the question. . . . The criticism that practice may influence individuals each by a law of his own, and processes each by a law of its own, does not seem to hold so far as the general law of improvement goes" (Tests for Individual Differences, 137–8).

Wells also is interested in the question of the extent to which individual differences "as we meet them in every-day laboratory experience, may be fundamental, inherent in the original nature of the individual, or may have been produced by special environment and training." His conclusions, in the case of a study of Addition and Cancellation tests, are as follows:

"We have then, finally, (1) a difference in the individual's (resp. function's) fundamental plasticity, *i. e.*, ability to profit by practice, (2) a difference in the actual amount of practice experienced, and (3) constitutional factors, independent of plasticity, in the nervous system. . . . In the present instances their influences would seem to operate in about the order named" (Relation of Practice to Individual Differences,—*Amer. Jour. Psychol.*, Jan., 1912).

The present study is concerned primarily with a combination of the problems of Whitley and Wells. To what degree are individual differences after a given number of trials indicative of the final maximal capacity of the individuals concerned? At what various rates do the factors enumerated by Wells enter into the practice curves of a group of workers, and what manner and amount of displacement in their relative abilities are thus produced? At what point or points in the curves do the individuals assume their final order of relative

capacity after training? How do the replies to these questions vary with the character of the test? By *final capacity* is here meant a degree of ability which, having been attained as the result of constant practice, remains practically unchanged by further practice during, say, 100 later trials. This seems to be a fairly correct description of what has been called, in the case of such tests, the 'physiological limit.' I shall later point out the meaninglessness of this term in such a connection.

The experiment consisted in putting each of 13 individuals through 175 repetitions of 7 different familiar tests. The trials were controlled as thoroughly as possible with respect to such incidental factors as interim occupation, exercise, food, rotation of tests, temperature, illumination, and incentive and interest. The subjects, four women and nine men, ranging from 18 to 39 years in age, were mature, and zealous, and competition was stimulated by the award of desirable prizes. Records were announced to the subjects only after each 35 trials. So far as previous practice in these particular tests is concerned, all the subjects were naïve. Five trials were made daily, these trials being distributed through the day at about two-hour intervals. The tests themselves occupied about 40 minutes at each trial (total for all subjects).

The tests were as follows:

1. Adding,—17 mentally to each of 50 two-place numbers and reciting aloud the correct answer. Order of numbers random at each trial. Record with stop watch,—time taken.

2. Naming Opposites,—correctly, of each of 50 adjectives which occurred each time in random order. Record,—time taken.

3. Color Naming,—the Columbia laboratory form of this test, with 10 repetitions of each of 10 colors. Position of card changed at each trial. Record,—time taken.

4. Discrimination Reaction,—discriminating between red and blue, and reacting with appropriate hand. Record,—sigma.

5. Cancellation,—crossing out digits from the Woodworth-Wells form of this test. Record,—time taken for 75 correct cancellations of equally difficult digits.

6. Coördination,—the familiar 'three-hole' test, for accuracy of aim. Record,—time for 100 correct strokes.

7. Tapping,—executing 400 taps at maximal speed, with hand stylus, right hand, elbow support. Record,—time taken.

Record has been here taken of the following points in the curves of practice:

1. Preliminary trial.....called initial trial
2. Median of regular trials 1–5.....5th trial
3. Median of regular trials 20–25.....25th trial
4. Median of regular trials 46–50.....50th trial
5. Median of regular trials 76–80.....80th trial
6. Median of regular trials 126–130.....130th trial
7. Median of regular trials 171–175.....175th trial

TABLE I

CORRELATIONS OF ORDER OF POSITION OF THIRTEEN INDIVIDUALS BEFORE, DURING AND AFTER PRACTICE

The correlation is in each case with the final order, after 175 practice trials (in two cases 130 trials). All coefficients are positive except where otherwise indicated.

The Test	Prelim. Trial	5th Trial	25th Trial	50th Trial	80th Trial	130th Trial	175th Trial
Adding	.154	.193	.874	.869	.973	.962	1.000
Opposites	−.088	.616	.490	.835	.945	.984	1.000
Colors	.682	.891	.858	.913	.968	.968	1.000
Discrimination	.676	.621	.604	.500	.500	.785	1.000
Cancellation	.665	.676	.885	.686	.934	1.000[1]	.....
Coördination	.528	.793	.770	.902	.946	1.000[1]	.....
Tapping	.231	.484	.627	.682	.693	.885	1.000
Averages	.41	.61	.73	.77	.85	.92	1.000

At all of these points the 13 subjects were arranged in order of relative ability for the test at the given stage of practice. Each of these orders, or cross sections of the group of practice curves, was correlated with the final order of position as shown in trials 170–175. Table I. gives the coefficients of correlation derived in this way, by the formula $r = 1 - \frac{6\Sigma d^2}{n(n^2 - 1)}$. A careful study of this table is instructive.

It is at once evident that the preliminary trial is in no sense a measure of the final relative capacities of the individuals tested. *Opposites*, *Adding* and *Tapping* give correlations which are practically zero, *Opposites* indeed yielding a

[1] Not included in average.

coefficient which is actually negative. *Coördination* stands considerably higher (+.53) but in no case does the correlation with the final order exceed +.68. *Color Naming*, *Cancellation*, and *Discrimination* are quite alike, giving coefficients of +.68, +.68, and +.67. In fact it is not until the 80th trial is reached that the majority of the coefficients rise to +.90 or over. Even here one is but +.50 and another +.69. Not until the 130th trial, indeed, do all the coefficients become +.80 or over. The average of all 7 coefficients increases from +.41 at the preliminary trial to +.92 at the 130th, as follows:

	Prelim.	5th	25th	50th	80th	130th	175th
Av.....	+.41	+.61	+.73	+.77	+.85	+.92	+1.00

As the trials proceed then, the relative positions of the 13 individuals become more and more definitely fixed. The rate of this process however varies from test to test, and that considerably. *Adding* shows changes in position which effect a correlation of +.87 only after the 25th trial. Beyond this point there is little change, the 80th and 130th trials correlating equally well and practically perfectly (+.97) with the final order. After 25 trials, then, the final capacities of the individuals in the *Adding* test may be said to be indicated fairly accurately. *Opposites*, in the 50th trial, yields a coefficient equal to that of *Addition* in the 25th, and by the 80th trial the correlation may be said to be complete. Only after 50 trials, then, can the test be said to yield comparative measures which reflect the final capacities of the individuals in this form of controlled association.

Color Naming, *Discrimination*, *Cancellation* and *Coördination* show up to much greater advantage. Even the preliminary trials in these tests show fairly high correlations with the final orders (+.68, +.68, +.67, and +.53). With *Color Naming* this degree of correspondence increases gradually, but the 5th trial (+.89) in this test gives as good an indication of final capacity as does the 25th trial in *Adding* or the 80th in *Opposites*. *Discrimination* shows, on the contrary, a uniform decrease from +.68 in the preliminary trial to only +.50 in the 80th, and even the 130th is only slightly higher than the 1st. In the cases of *Cancellation* and *Coördination* only 130

trials were made, and the correlations are with the last 5 trials (126–130). There is, in both cases, a gradual increase as practice proceeds, in the coefficients in these two tests. In the case of *Tapping*, and quite unexpectedly to the writer, it is only at the 130th trial that the correlation with final position exceeds +.69.

In general, if we assume a coefficient of +.75 to constitute the minimum degree of correspondence with final order required for the satisfactory practical determination of the relative capacities of the members of a group, the various tests yield this coefficient at the following points:

Test	Point Where r is at Least +.75
Color naming	5th trial
Coördination	5th trial
Cancellation	25th trial
Adding	25th trial
Opposites	50th trial
Tapping	130th trial
Discrimination	130th trial

Or if a correlation of two thirds (+.67) instead of three fourths be taken as the minimal desirable, the points are as follows:

Test	Where r is at Least +.67
Color naming	1st trial
Cancellation	1st trial
Coördination	5th trial
Adding	15th trial
Opposites	50th trial
Tapping	50th trial
Discrimination	130th (also 1st, but not maintained)

Except with *Opposites*, *Coördination* and *Tapping*, a preliminary trial is as reliable as is the median of the five trials just following it. Except for *Adding* and *Tapping*, five trials after a preliminary trial give correlations of +.60 or over with the final order. Only between the 25th and the 50th trials do the average correlations reach +.75 and an average coefficient of over +.90 is not reached until the 130th trial.

The meaning of these figures seems to be that before one attempts to interpret individual differences as disclosed by performance in such a series of simple tests, he should have clearly in mind the distinction between temporary proficiency

and ultimate capacity. If he is interested, for example, in determining the vocational prospects of a youth, or the relative merits of candidates or culprits, it is important that he realize that relative abilities in many of these laboratory tests may be changed quite beyond recognition by continued work. It is highly desirable to know more than we do about the degree to which initial and intermediate trials in these tests reflect final capacity. In the past the question seems hardly to have been asked. Individual differences in early trials in some tests are fairly significant of the working level to which the performer may be brought later. In other tests this is not the case.

Indeed there is little evidence that even the final level maintained for 100 trials or more in these experiments represents what may be called, in any correct sense, a physiological limit for the individual concerned. This level may represent the 'best he can do' under the circumstances, but so did the first trial, and the second, and every other. The limit in these earlier trials was just as 'physiological' as that after 175 trials. The actual processes of articulation and movement (or of mere reading aloud) may be made much more quickly than any of these individuals have been able to perform the tests involving articulation. Each level may, indeed, constitute a 'psychological limit,'—that is, the maximal efficiency which will be attained on the basis of the present incentive. But additional incentive, such as hunger or filial devotion might change notably the relative positions of the individuals. At this point in the curve of practice only a slight absolute change of level is required to bring about such shift in relative position. And it is measurement by relative position in one's group that is most likely to be of practical consequence.

It would be of interest to determine to what degree these changes in relative ability through the medium of practice are due merely to qualitative changes in the tests. *Opposites*, for example, and *Adding* show preliminary trials which do not correlate closely with the final orders of capacity. It is probable that with some individuals these tests come, after practice, to resemble the *Color Naming* test in character. The process would then involve less and less of the element of choice or

selection and the test would tend to be performed by rather automatic association between the stimulus and the response. Such a change would account for the failure of the first trials to correlate with the last only in case the change came more quickly with some performers than with others,—with some after a few trials, with others not until after some 50 trials.

Change in the nature of the tests, variations of methods of attack, and specific improvement in the directness, independence and rapidity of the special nervous connections concerned,—these three factors would all show up in the results in the form of "changes of ability." A useful piece of work in the case of all tests will be the analysis of the changes resulting from practice. But in any case the presence of these changes in correlation shows that we are not, in early trials, measuring the same thing with all performers. The concrete tasks of daily life doubtless show just such qualitative changes, during practice, as we may suppose to be present in some of these tests. Just as it is ultimate capacity in daily life that is, with a given set of incentives, most important, so in the laboratory the measurement of 'ability after practice' ought to be more emphasized than it is at present.[1]

[1] Cf. a related article, "Correlation of Abilities as Affected by Practice," H. L. Hollingworth, *Jour. of Educ. Psychol.*, Sept., 1913.

A TIME EXPERIMENT IN PSYCHOPHYSICS.

PART II

BY DARWIN OLIVER LYON AND HENRY LANE ENO

Part I. of this article appeared in the PSYCHOLOGICAL REVIEW for July, 1912. At the close of that paper, the authors stated that it was their intention to continue the experiment by the use of other methods—different both as to the treatment of the data, and as to the nature of the stimuli. The present article embodies not only the results thus obtained, but attempts, also, to answer and explain the various criticisms and objections that have been developed since the publication of the first article. No effort will here be made to repeat the defense of the nine 'possible sources of error' mentioned in Part I.,[1] although a few words further will be said upon electrotonus and kindred phenomena.

An entirely new apparatus was constructed, utilizing, by way of improvement, any additions or modifications that experience with the old apparatus had shown to be desirable. An additional apparatus was made, also, for the giving of tactual in the place of electrical stimuli.

DESCRIPTION OF THE NEW APPARATUS

Although especially constructed on an entirely fresh plan, the former mechanical method of giving the two successive stimuli was retained. For a detailed description of this part of the apparatus, the reader is referred to the first article. The main changes made in the later machine were such as would assure great accuracy in its running; the new devices securing a minimum variation in the rotary speed of the disks, with the consequent ability to measure accurately the exact time interval between the various pairs of stimuli. A fair

[1] Lyon and Eno, 'A Time Experiment in Psychophysics,' Part I., PSYCH. REV., Vol. XIX., pp. 326–327.

idea of this new apparatus may be obtained by a study of Figs. 1 and 2.

In the old apparatus the disks were revolved by a falling weight; in the present machine they are driven by a one fourth horse-power dynamo. The current is supplied by a large storage battery. By changing belts or shifting the controls, a wide range of velocities can be obtained. The fly-wheel weighs over 200 lbs. and by its momentum insures great regularity in the rotation of the disks. The number of revolutions per minute is read from a tachometer. Even after several hours of running, no variation of speed is observable, and in every way the actual working of this apparatus is highly satisfactory.

In order to do away with any objection or explanation of the 'seeming discrepancy' in our results, having as its basis a change in the *electrical condition* of the nerve, be it electrotonus, or what not, as well as to compare the results obtained with some stimulus other than electrical, an apparatus was constructed by which tactual stimuli could be given.

This tactual or 'hammer apparatus,' as we have called it, may be seen in Fig. 1, resting on the small table. The essential part of it consists of two small metal hammers one half inch in diameter and tapered to a point, as shown in the photograph. The hammers are controlled by small coils which, in turn, are attached to the disks in the same manner as are the electrodes that give the electric stimuli, and thus, like the electrical stimuli, the hammer strokes can be separated by any desired interval of time. By altering the amount of the current, the force of the strokes may be increased or diminished at will. As may be seen in the photographs, the coil and hammers are fastened to sliding boards set upon an adjustable arm rest.

By the experiments described in the previous article it was shown[1] that if two electric stimuli of like intensity were applied to the musculo-cutaneous nerve at points some eight inches apart,—the most convenient points being at the wrist and just below the elbow—the stimulus at the wrist (St^1) had to be given about one fortieth of a second before the stimulus

[1] *Op. cit.*, pp. 318–326.

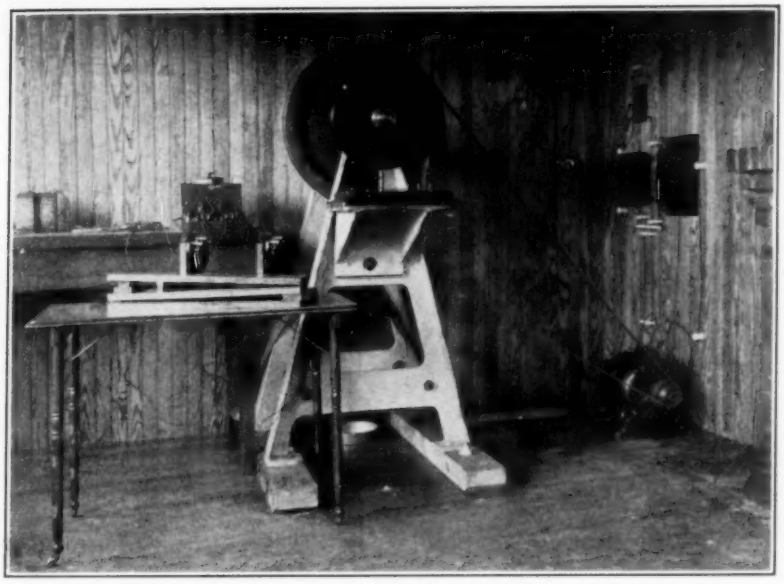

FIG. 1.

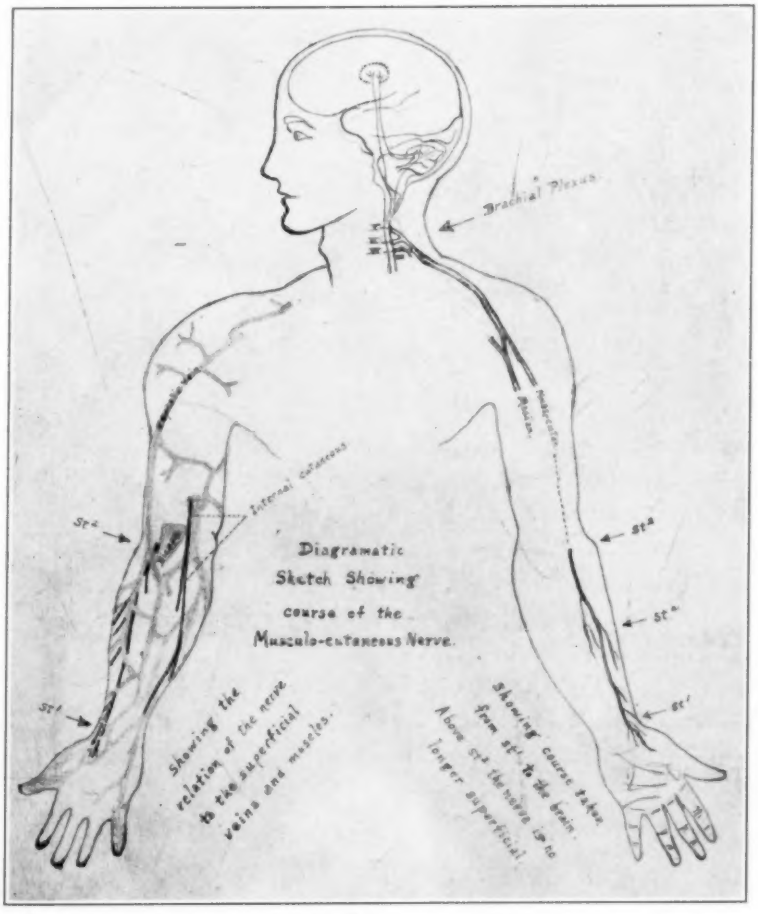
Brachial Plexus
Internal cutaneous
St.a
St.a
St.c
St.f
Diagramatic
Sketch Showing
course of the
Musculo-cutaneous Nerve.
Showing the
relation of the nerve
to the superficial
veins and muscles.
Showing course taken
from St.f to the brain.
Above St.a the nerve is no
longer superficial.

FIG. 2.

at the elbow (St^2) if it was desired to make the two sensations appear simultaneous to the observer. If the speed of the nervous impulse, as generally accepted, was even approximately correct, this interval is fully three times as long as we should expect to find it.

The method employed was to gradually increase the interval between the stimuli until the subject felt them as separate in time, *i. e.*, one before the other. A converse method was also used whereby, starting with stimuli that were obviously non-simultaneous, the time interval was gradually shortened until the two stimuli appeared to occur together.

If the results of the preceding experiment are correct, it follows that, since St^1 must be applied 1/40 sec. before St^2 for the two stimuli to be felt together, if St^1 is applied at any *less* interval than 1/40 sec. (as 1/60 sec.), before St^2, the observer should be aware of St^2 first by a small fraction of a second, although St^1 is actually given beforehand.

In our earlier experiments as described in Part I. we used a modification of the 'method of just noticeable differences,' endeavoring by 'working from both directions'[1] to determine, as nearly as possible, the interval at which the two stimuli had to be applied apart, for them to appear together in consciousness. This interval was found to be on the average .0263—a trifle over 1/40 of a second. The method used had the advantage of speed and simplicity, but we desired to substantiate our results, if possible, by a more accurate method. What, for example, would the *majority* of subjects in the *long run* answer for stimuli given at .0333 of a second apart? We know from the experiments described that when St^1 is given .0412 sec. before St^2, St^1 is felt before St^2. The reader might justly assume that with an interval of .0400 sec. St^1 would *generally* be felt before St^2, but not always—since they *sometimes* would seem to be simultaneous at this interval. It was to discover the percentage of 'right'[2] judgments in such cases, and thus

[1] *Op. cit.*, p. 321.

[2] In one sense of the word, with an experiment like the one in question, there can be no such thing as a 'wrong' judgment. What we mean by 'wrong' is that, had the subject's attention, nervous condition, etc., been normal, he would not have made this 'out of the ordinary' judgment.

arrive at a more exact 'period of simultaneity,' that the 'method of right and wrong cases' was used.

If, after a large number of observations, a subject made no error when the stimuli were separated by a certain time interval, we would be justified in assuming that he could really distinguish one sensation before the other without guessing. By making the time interval smaller, we would naturally be justified in assuming that, although he could still, in the main, tell one sensation before the other, he might, now and then, make a 'wrong' judgment, *e. g.*, he might say that the two sensations appeared to occur at the same time. By making the time interval still smaller, so small, for example, that he was only right three fourths of the time, the subject would be noticing differences much smaller than he did when he was invariably right. Thus by setting the disks in such a way that the subject is sometimes right and sometimes wrong, we have our 'method of right and wrong cases.'

In applying this method to our experiment, we were met with certain difficulties. In the first place, we had no positive objective criterion or 'standard' to go by, for as already explained, there are no means of determining before each and every trial what the exact time interval should really be in order to have the two sensations appear synchronous.

Another difficulty is that we here have three possible contingencies: St^1 may be felt before St^2, St^1 may be felt after St^2, St^1 and St^2 may be felt together. We found, however, that the most satisfactory method was to set the disks in such a way that St^1 was given 1/60 sec. before St^2. With this interval, although the two stimuli might appear synchronous during some observations, yet the majority of observations would give St^2 as occurring before St^1.[1] With intervals as small as this, all three of the possibilities would obviously be sometimes obtained. Our method, however, was to disregard the exact nature of the answer and merely require the subject to say which of the two stimuli appeared to him to be felt first. No 'synchronous' answers were allowed, and we always

[1] When St^1 is applied 1/60 sec. before St^2, St^2 is nearly always felt first. With this interval the proportion of wrong to right results is as 1 : 6.

insisted on the subject making a *guess*, knowing that in the long run the number of right guesses would be more apt to be right than the number of wrong guesses.

Space does not permit a presentation of the data, nor is this necessary. Suffice it to say that the results obtained by this method were much the same as those obtained by the old method, viz., that in order to get St^1 and St^2 to appear together in consciousness, St^1 had to be given from 1/80 to 2/80 sec. before St^2, the average being 1/40 sec.[1]

Experiments were made with the hammer apparatus as well as the electrical apparatus, but the results in each case were identical. In both cases when St^2 is applied 1/60 sec. after St^1, St^2 is felt first, and the reaction experiments conclusively show that the nervous impulse cannot travel slowly enough to obtain such a result.

Possible Sources of Error

In the preceding article (Part I.) nine 'possible sources of error' or 'explanations of the apparent discrepancy' were discussed, explained, and dismissed to the best of the authors' ability. At the time of writing (1912), they were the only 'explanations' that had occurred to either of us. Since the publication of the article in question, three new 'possibilities' have either suggested themselves, or been called to our attention.

We also feel that 'Objection No. 9'—that referring to a possible change in the electrical condition of the nerve—was not fully answered. We therefore take this opportunity of adding a few words further on this point—after which we shall consider the three new 'possible explanations.'

In our previous article the question was raised as to whether the first stimulus might not, by causing a change akin to *electrotonus*, result in a retardation or acceleration of the propagation of the second stimulus. With the first contingency we have nothing to do, for the simple reason that St^2 is not retarded; on the contrary, it would appear to be hastened.

Strictly speaking, *electrotonus* is the modification of irrita-

[1] This is the average obtained from over five hundred tests on thirty subjects.

bility of a motor nerve caused by the passage through it of an electric current. The condition holds to a certain extent for the sensory nerves, but not to so great a degree. Though a similar condition is caused by a single induction shock, the laws of electrotonus, as they are generally formulated, presuppose a *constant* current, a factor that does not enter in our experiment. During the passage of a constant current through a nerve, the irritability of the nerve is increased in the region of the cathode, while at the anode it is diminished. The change in the nerve that gives rise to this increased irritability in the region of the cathode is spoken of as catelectrotonus, while in the region of the anode the change is known as anelectrotonus. This law, if we may so term it, remains true for each and every method of determining the changed irritability, be it a single induction-shock, an interrupted current, a mechanical or chemical stimulus, and it holds true not only for the so-called 'muscle nerve preparations,' but also for the intact nerves as they lie in the living body.

The results derived from the present series of experiments would, however, seem to eliminate the possible action of electrotonus as a source of error in this case.

We have found that St^2 is felt first, even when St^1 is given 1/60 sec. beforehand. Therefore, if this result is to be explained by electrotonus, either the nervous process set up by

FIG. 1.

St^1 must be retarded, or that set up by St^2 accelerated, sufficiently to account for the discrepancy.

To put it graphically:

If we call the point of application St^1 *A*, that of St^2 *B*, and the cortex *C*—

1. The process from St^2 must travel from *B* to *C* in less than 1/60 sec. while the process from St^1, after taking *more* than 1/60 sec. to go one third of that distance from *A* to *B*, takes *less* than 1/60 sec. to accomplish the three times longer path from *B* to *C*, *i. e.*, it travels at not only a much slower, but also an uneven rate; or,

2. The process from St^1 must be retarded by more than 1/20 sec. before reaching *B*, and that *before* St^2 is given at all; or, if the process from St^1 is not retarded,

3. The process from St^2 must be accelerated in such a way as to overtake and pass the process from St^1 on its way between *B* and *C*.

Now it is difficult to see how electrotonus could cause any of these effects.

Condition 1 is impossible, because, we know from other reaction experiments that the process from St^1 travels from *A* to *C* in, at the most, 2/60 sec. Therefore, although it may take *less* time in transition, it cannot take more.

2. It is inconceivable that the process from St^1 can take *more* than 1/60 sec. to travel over the free path from *A* to *B*, and less than 1/60 sec. to go from *B* to *C*—a path involving the brachial plexus, medulla, thalamus, and possibly parts of the cortex itself.

3. It is also improbable that one nervous process starting after another nervous process, and travelling in the same path, can overtake the first process and pass it, without some kind of fusion.

The most conclusive proof, however, that neither *electrotonus*, nor any other kindred electrical phenomenon, lies at the root of the explanation, is that the same results are obtained with the hammer apparatus, which gives tactual stimuli only.

Of the further possible sources of error, the first[1] that we shall consider is the possibility that nervous impulses from the two points of stimulation end at different cortical 'levels' instead of proceeding to the same place; in short, that the impulse given by St^1 involves a greater distance—be it anatomical or physiological—than the impulse from St^2. Graphically this explanation might be shown thus

FIG. 2.

where S^1 and S^2 refer to the corresponding sensory areas.

[1] As a possible explanation this criticism was brought to our attention by Dr. Head, editor of *Brain*, and by Professor H. C. Warren, of Princeton University.

In answer to this objection we would say that what little is known of the anatomy of the sensory tracts shows no indication of any such arragnement. Not only have we no reason for assuming any such anatomical state of affairs—but, run to the extreme, such an assumption would mean that the greater the distance away from the brain at which a stimulus is given, the shorter is its path in the brain itself.

It is true that we still know comparatively little of the course of the sensory neurones after they leave the tegmentum and optic thalamus, but we have no reason for thinking that the centripetal impulses started at or near the periphery of a cutaneous nerve change in character as they travel inwards. It should be remembered that the possible paths for the conduction of afferent impulses are many, and that they become more and more complex as the various tracts approach the brain. It is thought that the greater part of the sensory impulses reach the optic thalamus, and from here are distributed to the various parts of the cortex, but of the exact anatomical arrangement we know almost nothing. The localization area of sensory impressions in the cortex is thought to be posterior to the motor area—but even of this we have no definite knowledge. All we know is that the sensory area seems to be less sharply defined than the motor area in that it occupies the greater part of the parietal lobe as well as the posterior central convolutions. Seeing how unsatisfactory is our knowledge with regard to the sensory tracts in the relatively simple spinal cord, and how little we know of their distribution between the medulla and the thalamus, it is obviously useless to attempt to form any anatomical explanation of the 'apparent discrepancy' when we know practically nothing at all of the final distribution of the tracts in question. All we can say is that the little which embryology can tell us points against any such explanation.

Moreover, a brief reference to the well substantiated results of reaction-time experiments will show that no such difference in time between the sensations corresponding to two stimuli, such as we are considering, can be due to the traversing of respectively longer and shorter paths in the

brain by the nerves involved. For if an increasing difference of 1/40 second occurs for every 8 inches in the length of a nervous path, as the points of stimulation are taken further from the brain, a reaction time from the neck should, roughly speaking, occur in a time equal to the reaction time from the hand less a proportionate time, at the rate of 1/40 sec. for each 8 inches of the additional distance to be traversed. Thus for the extra 32 inches, it would be 1/10 sec. less 4/40 sec. That any such explanation is impossible is evident when we calculate the reaction time of the neck on this basis—which would be 1/10 − 1/10 or 0.

In like manner a reaction time of the foot would consume an altogether disproportionate interval, since a touch reaction time rarely rises about 2/10 second. But, as it is well known, there are no parts of the body which, when stimulated, show reaction times which are either abnormally long or nearly instantaneous.

Furthermore, if a stimulus be given at the shoulder with one of the hammers, the other hammer can be seen to move *before* the sensation at the shoulder is felt, when the hammers are timed in such a way that the hammer at the shoulder strikes 1/60 sec. before the hammer which is visually perceived only. Therefore, if a brain process and its correlative sensation were simultaneous, the nervous impulse from the shoulder must take at least this 1/60 sec., plus the unknown time (X) which the visual impulse consumes, to reach its appropriate sensory area.

Now if a further time of 1/40 sec. for each 8 inches is added as the stimulus is given further down the arm, it would take 1/60 sec. plus, as before, the time X, to which must be further added 3/40 sec. (for the additional 24 inches from shoulder to hand), or almost 1/10 sec. in all for a nervous impulse from the hand to reach the cortex.

But a *reaction time* from the hand[1] can occur in 1/10 sec.; hence there is only 1/10 − 1/10 or 0 seconds left, in which the

[1] The hand-to-hand reaction time includes whatever time is required for the transmission of the efferent as well as the afferent nervous impulse, together with muscle innervation, etc., which, of course, makes the comparison even more absurd.

association processes, efferent impulse, and muscular innervation must all take place—which is palpably absurd.

Furthermore, that no such difference as 1/10 sec. exists between the sensations from the hand and eye is easily proved by direct experiment, for if the hammers are timed so that the strokes occur 1/10 sec. apart, the touch of the first hammer is distinctly and invariably felt, by all observers, before the second hammer is seen to move.[1]

It would seem, therefore, that there cannot possibly exist any such retardation of the nervous impulse from St^1 due to its proceeding to some endpoint involving an additional cortical path beyond the endpoint reached by the nervous impulse from St^2.

Another objection raised is that, in an experiment where a discrimination between two successive sensations is involved, additional 'perceptual' centers may also come into play, as p^2 and p^1 in the diagram below.

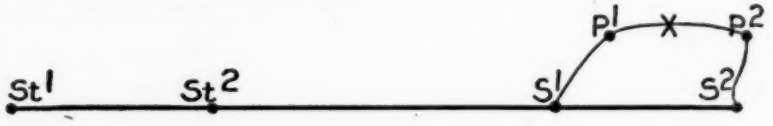

FIG. 3.

These perceptual centers would, further, be connected by an associational path (x), and the longer time taken for St^1 to get itself perceived in comparison with St^2 might depend on the additional cortical area, which must be brought into action before the process set up by the nervous impulse from St^1 emerges into consciousness.

In answer to this objection, it is only necessary to point out that it makes no difference *what parts of the cortex or brain* the impulses from St^1 and St^2 eventually reach and involve as the correlative substrata of their respective sensations or perceptions.

The considerations indicated in the answer to the first objection show that it is impossible that any such discrepancy in time between the perceptions of the two stimuli could be taken up through the mere implication of different areas.

[1] The 'smallest interval' between sight and touch is rarely more than 1/20 sec. Ladd & Woodworth, 'Psychology,' p. 475.

For any discriminative reaction from the two stimulated points in question necessarily involves those same perceptual areas, and a discriminative reaction can be gotten in .18 sec. which would be impossible if the conditions urged in this objection obtained; nor could the time interval between stimuli from two different points, as from the two hands, where both hemispheres of the brain are involved, be distinguished, as it is well known to be, at such minute fractions of a second as 1/360.

The involving of different areas as the essential concomitants of these two sensations or perceptions, or both, if they can account for the time difference in question, must do so either because the specific character of the area affected by St^1, or the mere fact that *different* areas are involved, always retards the sensation of the stimulus which is given first.

We have seen that a longer path does not furnish an adequate explanation. Nor can the cerebral effect of the first stimulus always be retarded; for, in that event if St^2 should be given first by any interval (say 1/60 sec.) sufficient to overcome the time taken by the nervous impulse to travel from the points St^1 to St^2—in that event St^2 should be felt first. But, as we have shown, this is not the case.

A further objection should also be mentioned briefly.[1] It is the general criticism that our present knowledge of what actually happens in the brain is so vague, that we know so little what areas are involved as substrata of any given mental process, that conclusions drawn from the localization of definite sensory or perceptual tracts or the nature of any particular processes, must necessarily be untrustworthy.

As we have seen, however, in the discussion of the preceding objection, neither exact localization nor accurate description of process are essential for the validity of our experimental results. They hold good whatever areas may be involved, or of whatever nature the cerebral processes may, ultimately, be found to consist.

[1] This objection was called to the authors' attention by Dr. Wm. McDougall, of Oxford, and by Professor Henri Bergson.

Summary

All the possible physiological causes that might explain the different relative times which our experiments show to be consumed by the nervous impulses from a stimulus at the wrist (St^1) and a stimulus at the elbow (St^2) in reaching and setting up the appropriate cerebral processes correlative to their respective sensations must fall under one of two headings, either:

1. The impulse from St^1 must be retarded, or
2. The impulse from St^2 must be accelerated.

This acceleration or retardation, also, must be due, either

(*a*) To the physiological conditions at the point of stimulation, nerve path, or brain area involved, in each case, or,

(*b*) To the fact that St^1 is set back because it is given first in time, or St^2 set forward because it is given after St^1.

In the preceding article and the present paper, we have pointed out that there seem to be not only no anatomical or physiological grounds of explanation, but that it can be shown experimentally, by a comparison with known reaction times, that no such retardation or acceleration can exist.

It has been demonstrated by actual experiment, also, that whichever stimulus is given first in time, the results are not materially altered.

Conclusion

In a series of experiments extending over two years, and carried on by means of various apparatus and methods which, as far as the authors can ascertain, have thoroughly substantiated the results, it has been discovered that when two successive stimuli, either electrical or tactual, are applied at different points over the same nerve, there exists an altogether disproportionate interval between the respective times of stimulation and the occurrence of the correlative sensations.

The two points of stimulation chosen for the majority of the experiments were upon the musculo-cutaneous nerve at the wrist and just below the elbow. These points are, in general, about eight inches apart, but the time elapsing between the application of the stimulus at the wrist and its corre-

sponding sensation is some 1/40 sec. longer than the elapsed time between stimulus and sensation when the stimulus is given at the elbow.

As the velocity of the nervous impulse is at least thirty meters a sec. and, therefore, cannot take more than 1/20 sec. to traverse the eight inches between the points of stimulation, the unaccountable discrepancy between the times of occurrence of the corresponding respective sensations is approximately 1/60 sec.

In searching for an explanation for this discrepancy, we have examined in detail, in the present and preceding articles, various possible sources of error which can be roughly grouped under the following headings:

A. Anatomical. Under this heading are included:
1. Difference in the sensitivity of the skin at the two points of stimulation.
2. Difference in depth of nerve at the two points.
3. Number of synapses and nerves involved.
4. Different perception centers.

B. Physiological.
1. Fusion of successive impulses in any portion of the nervous system—central or peripheral.
2. Frequency with which the two nerve tracts involved are accustomed to transmit stimuli.
3. Difference in velocity in different parts of the neurone.
4. Electrotonus and kindred phenomena.

C. Psychological.
1. Inaccuracy of observation.
2. Effect of attention on any part of the neurone, including the so-called 'hair-trigger' condition, monopoly of the subject's attention by the first stimulus, etc.

As a result of the examination of the above possible sources of error, we have been unable to find that they adequately explain so great a discrepancy in the time intervals between the two stimuli in question and their respective correlative sensations. Since, however, the experiment would appear to indicate that, when the two stimuli are so timed that the corre-

sponding sensations occur simultaneously, the correlative cortical processes *do not* occur simultaneously, it would seem to follow that, in the case of sensation, at any rate, the cortical and psychic processes are not *synchronous;* but that the cortical process precedes its correlative psychic process by a small, but not experimentally imperceptible, interval of time.

THE EFFECT ON FOVEAL VISION OF BRIGHT SURROUNDINGS—II

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The material of the present paper is a continuation of work recently reported.[1] The apparatus, the technique of procedure and the method of computation of results are identical, so details will be treated here only in so far as they vary from those of the previous work.

One of the two observers is the same for both pieces of work. Since the two alternated as experimenter and observer, it is natural that the technique of procedure would be modified, in a certain incalculable way, by the replacement of the second individual. Every essential detail, however, was kept as nearly as possible the same as it was before.

The only essential variation in conditions was the substitution of surroundings for the test object of brightness 2.87 candles per square meter, in place of surroundings of brightness 41.9 candles per square meter, which is approximately of 1/15 the brightness formerly used. Further, instead of completing the observations with dark surroundings before beginning those with bright surroundings, as was done before, the two were carried out over the same period of time. This manner of procedure is obviously preferable to the other, but was not carried out before owing to delay in the construction of the apparatus used to make the surroundings.

Incidental Observations

Reference was made in the previous communication to certain disturbances experienced when the test objects were highly illuminated and observed in dark surroundings. Observer *J* in these experiments experienced some disturbance under the same conditions. At intensity *a* there was a glare

[1] Psychological Review, XX., pp. 425–447.

always disagreeable and sometimes painful, the outlines of the object often becoming blurred. This disturbance was greatly decreased under bright surroundings. At intensity *c* under bright surroundings the conditions were exceptionally pleasant. There was some discomfort at intensity *e* with

TABLE III

Brightness of Test Object			Surroundings Dark							Surroundings, 2.87 Candles per Sq. Meter						
Designation	Candles per Sq. Meter	Log of Same	Relative Visual Acuity	Mean Var. P. C.	Visual Angle Minutes	Diff. Limen Per Cent.	Mean Var. P. C.	Mixed Region Per Cent.	Mean Var. P. C.	Relative Visual Acuity	Mean Var. P. C.	Visual Angle Minutes	Diff. Limen Per Cent.	Mean Var. P. C.	Mixed Region Per Cent.	Mean Var. P. C.
a	83.35	1.921	5.36	1½	0.50	0.26	86	1.71	27	5.27	2	0.51	0.42	65	1.29	35
			3.87	6	0.70	0.50	52	1.34	34	3.95	4	0.68	0.50	58	1.49	25
b	14.8	1.171	4.89	2	0.55	0.16	126	1.21	60	5.08	4	0.53	0.32	75	0.94	37
			3.52	4	0.77	0.30	81	1.38	48	3.65	6	0.74	0.51	51	1.13	39
c	2.90	0.462	4.26	2	0.63	0.35	78	1.09	50	4.33	2	0.62	0.25	84	0.80	53
			3.24	6	0.83	0.21	78	.99	40	3.18	6	0.85	0.26	93	0.80	37
d	0.593	9.773	3.58	4	0.75	0.15	124	1.05	46	3.69	6	0.73	0.59	99	1.43	76
			2.77	4	0.97	0.48	72	1.54	50	2.59	5	1.04	1.21	40	2.92	26
e	0.176	9.244	2.89	4	0.94	0.31	117	1.45	49	2.55	4	1.06	1.39	64	4.23	52
			2.36	6	1.14	1.21	76	1.88	81	2.06	5	1.31	4.04	34	6.67	36

The upper figures in each case give the results for observer *J*, the lower for *C*.

bright surroundings. The apparent brightness of the respective halves of the field, as well as the lines on the acuity object, would frequently appear to shift during exposure of the test object. Judgments at this intensity were more uncertain than at any other.

The results are given in Table III. and embodied in Figs. 7 to 11 where, as before, the abscissæ (brightness of the test object) are in every case plotted as logarithms, and the ordinates as the actual difference per cent. or as minutes visual angle in the case of visual acuity.

INDIVIDUAL DIFFERENCES BETWEEN THE OBSERVERS

The present results show that in vision both at absolutely low brightness of the test-object and also where the latter was low only relatively to the surroundings, *J* has better discrimination than *C*, the difference being quantitatively about

the same as the difference shown between *G* and *C* in the preceding work, although the curves in the present case do not so readily admit of a numerical estimation of this difference. Further, it appears that at all points, with a few exceptions, *J* showed better discrimination than *C*. This is most clearly evident in the case of visual acuity (Fig. 9) where *J* shows a

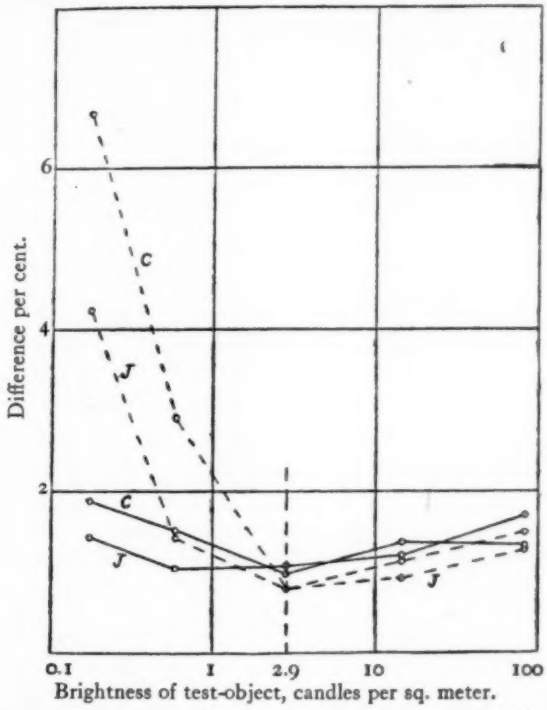

FIG. 7. Extent of the Region of Mixed Judgments (*M*-values) for the Two Observers under Various Conditions. Dark surroundings ————————. Surroundings 2.9 candles per sq. meter – – – – – – – – – – –.

better value by a ratio of about 7 to 5 throughout. In the case of the *M* values (Fig. 7) and the difference limen (Fig. 8) there are a few exceptions, but on the whole the difference is seen from the curves to be a general one.

DIFFERENCES DUE TO THE SURROUNDINGS

As before the bright surroundings are seen in all cases to cause more or less extreme loss in visual discrimination when

the test object is observed at a brightness much less than that of the surroundings. At the point of equality and in the cases where the test object is seen in surroundings less bright than itself, the results are by no means so decisive. For instance in Fig. 8 the difference-limen at *a* and *b* (test object 83.3 and 14.8 candles per square meter) is for both observers greater with surroundings of 2.9 candles per square meter,

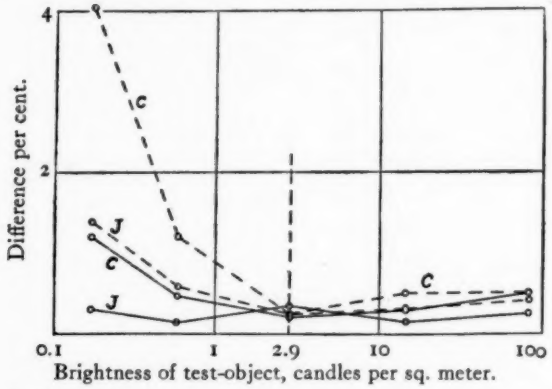

FIG. 8. Difference-limen (*L*-values) for the Two Observers under Various Conditions. Dark surroundings ——————. Surroundings 2.9 candles per sq. meter - - - - - - - - - - -.

than in dark surroundings, while the test object when observed at a brightness equal to these surroundings gives contradictory results with the respective observers. It is not easy to see why this should be, for if surroundings equal in brightness to the test object give results (at least) as good as those obtained with dark surroundings, it is hard to see why surroundings of brightness greater than zero and less than that of the test object should bring about inferior discrimination.

As to the *M* values (Fig. 7) these indicate greater consistency of judgment wherever the test object is of brightness equal to or greater than that of the surroundings than in the corresponding cases of dark surroundings. The one point to be noted as an exception is that of the highest brightness of test object in the case of observer *C*, where the result is just the reverse, and is subject to the same remark as that made in the last paragraph in discussing the difference-limen.

Visual acuity (Fig. 9) exhibits certain irregularities but the general course of the curves, and the comparatively small mean variations in the individual results, justify a somewhat briefer and more general conclusion from them, namely that bright surroundings which are not brighter than the test object itself result in slightly better vision than the dark surroundings. Surroundings which are bright much in excess of the test object give results less marked in the case of visual acuity than in the case of brightness difference, but neverthe-

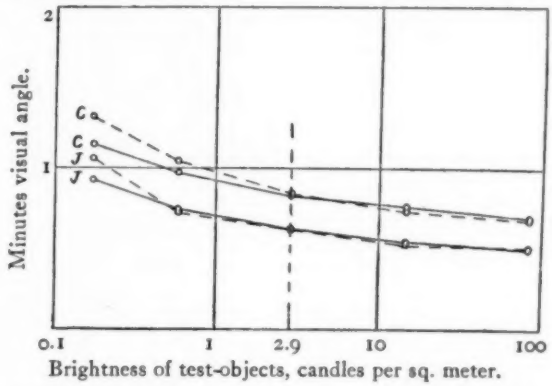

FIG. 9. Least Visual Angle (*V*-values) for the Two Observers under Various Conditions. Dark surroundings ————————. Surroundings 2.9 candles per sq. meter - - - - - - - - - - -.

less, plainly following the general rule that excessively bright surroundings interfere seriously with vision.

The mean results for both observers, *C* and *J*, are given graphically in Fig. 10. Fig. 11 is a summary of the difference limen and least visual angle for all three backgrounds; dark, 2.87 and 41.9 candles per square meter. The results of observer *C* only are used here since they are the only ones comparable under the three conditions.

DISCUSSION

Comparison of the visual acuity and brightness-difference curves under parallel conditions show that as the test-object brightness is reduced the difference-limen usually at a fairly definite point takes rather an abrupt rise. Visual acuity, on the other hand, while always showing a slight progressive

diminution beginning at the very highest brightness under a similar change of conditions, never undergoes such rapid decrease as differential sensitivity. This fact is to be considered in connection with the almost obvious fact that discrimination of fine detail depends upon (*a*) a physically perfect image on the retina and (*b*) probably upon the accurate fixity

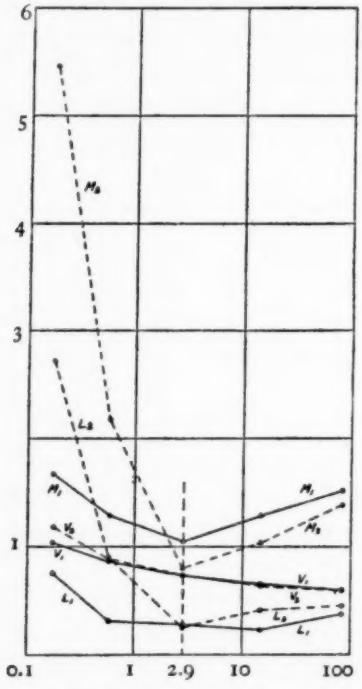

FIG. 10. Mean Values of *M*, *L* and *V* under Various Conditions. The ordinates represent difference per cent. in the case of *M* and *L* and minutes visual angle in the case of *V*. Dark surroundings ————————. Surroundings 2.9 candles per sq. meter - - - - - - - - - - -.

of this image which in turn depends on the steadiness of the extra-ocular muscles. Since there is nothing in uniformly bright or dark surroundings to influence either of these factors (except as noted farther on) it may be concluded that visual acuity depends mainly upon these, and hence varies less under the influence of contrast than does differential sensitivity because the retinal image is always equally perfect.

The exception to be noted to this last assumption refers to the effect of the size of the pupillary aperture on the sharpness of the image. The usual assumption on this point is that a smaller pupil causes a sharper retinal image and hence a better value for visual acuity. There are several considerations of pure physical optics which enter into this question, one of which speaks in a direction exactly contrary to the usual view

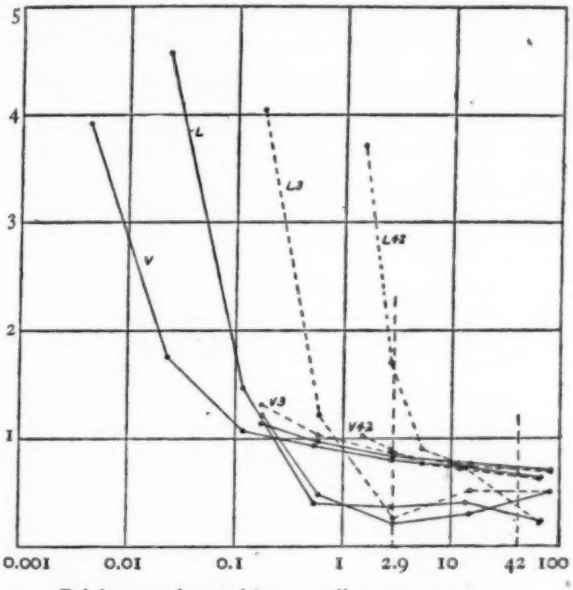

FIG. 11. All Values for *L* and *V* for One Observer (*C*). The ordinates represent difference per cent. in the case of the limen and minutes visual angle in the case of visual acuity. The figures following the letters designating the curves drawn in broken lines give the approximate brightness in candles per square meter of the surroundings used. The short curves drawn in solid lines represent the values for dark surroundings obtained in connection with the work of the present paper, the long ones those of the preceding work.

and a presentation of certain results is contemplated for the near future which show that under some conditions at least, a small pupillary aperture may result in inferior vision. Obviously without actual pupillary measurements the effect of the size of pupil cannot in any case be estimated.

As contrasted with visual acuity, differential sensitivity can be said to depend mainly upon retinal conditions, and to a

very minor degree upon the perfectness of the retinal image. The two forms of discrimination might be considered to be special cases of the same phenomenon rather than as different phenomena, since each involves perception of differences of both intensity and extensity. In the vision of fine detail, as well as in the perception of small brightness differences between simultaneously presented fields, there must be perceived differences in brightness which must at the same time be referred to areas differently localized. Otherwise no judgment could be made. The essential difference is that in the case of visual acuity estimation the brightness difference of the parts of the test object is gross, while the areas involved are minimal. Hence a slight mixing up of various parts of the image by dioptric irregularities of any sort produces a decided fall in visual acuity. Disturbing the refraction of a normal eye to the extent of one half diopter by the use of a convex lens brings about a decided decrease in visual acuity readily demonstrable by means of the Snellen test-letters.

On the other hand, in the estimation of differential sensitivity by simultaneous presentation the areas of the fields compared are gross while the brightness-difference is minimal. Small irregularities in the formation of the retinal image need not be expected to have any noteworthy effect in this case but the important factors would be those influencing the light sensitivity of the retina, namely the contrast-effect of the surroundings of the test-object, and the after-effect of previous stimulation of the retina, its state of adaptation and the presence or absence of after-images. The first named influence, that of the surroundings, is shown by the results to be far more significant for differential-sensitivity than for visual acuity. Investigation of the after-effects of previous stimulation of the eye was not contemplated in this work.

The explanation of the differences in visual capacity which attended the difference in surroundings as used in the present work must be referred in the first place to contrast. The most marked effect noted is what has been called by Abney the 'extinction' of light by the illuminated retina, where the surroundings are much brighter than the test-field. In the

opposite case, where the test-field is about equal to, or brighter than the surroundings the differences in vision due to these surroundings as against dark surroundings appear to be on the whole in the direction of better vision, although the smallness of the difference, the large mean variations in the case of brightness-difference estimations, the non-agreement of the results and certain factors in the technique used lay the results open to some doubt.

Aside from contrast there is another factor which undoubtedly plays a part in the depression of vision under very bright surroundings. The eye-media are not perfectly clear and every object within the visual field sends light into the eye, all of which except a probably minor fraction constitutes the retinal image. This small fraction is scattered within the eyeball. Further, the light constituting the image must undergo lateral diffusion within the retinal and subjacent tissues. This diffusion is probably far more extensive than the limits of what is known as irradiation. Just what the magnitude of illumination may be, due to the light from any particular object thus diffused to any part of the retina is not known, but it is abundantly demonstrable by the simple experiment of placing a light source somewhat eccentrically in a rather dark visual field, a few feet from the eye, and alternately shading and exposing the eye. It will be noted that when the eye is protected from the direct light of the lamp that almost the entire visual field is clearer as to detail and is darker, and that when the direct light is again admitted to the eye the field becomes at once brighter and more confused.

It is to be remembered that surfaces, such as white and black paper when seen under equal illumination, present brightnesses to the eye which are about as 20 to 1. In the two extreme cases of the present work the test-object was seen upon grounds of about 16 and 28 times its own brightness respectively. Neither ordinary vision of opaque objects by reflected light nor the conditions of the experiment just mentioned bear direct visual evidence of scattered light within the eye, since under these circumstances its effect is at least fully compensated by contrast, which acts in exactly the

opposite sense. Yet scattered light is there and its consideration is necessary to the understanding of visual phenomena.

Summary

The conclusions from the results just given are in general not substantially different from those stated in the preceding paper, except as qualified by the following statement which is to be taken in connection with conclusion (4) of the former paper:

Surroundings of a brightness about equal to or less than that of the test object show no consistently better or worse results than dark surroundings with the identical test object. On the whole, visual acuity under these circumstances was slightly improved and the difference-limen increased, but in the latter case the diffusion of the results (*M*-value) was distinctly diminished.

By a somewhat different method now under consideration it is hoped to eliminate some of the uncertainties inherent in the present one and obtain more definite results as to brightness-discrimination where the present results are unsatisfactory.

The writer wishes to acknowledge the coöperation of Dr. H. M. Johnson as experimenter and observer in this work.

THE EXPRESSION OF THE EMOTIONS

BY ANTOINETTE M. FELEKY

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It is the purpose of this article to illustrate the expressive movements characteristic of certain emotional states, or rather, to show what emotional states certain facial expressions do signify.

Several hundred photographs of the same individual, A. F., were taken at different times during a period of one year. As she posed for each photograph, A. F. had clearly in mind what she was endeavoring to portray, either by deliberately calling up the emotion itself, or by reciting words expressing the desired emotion.

Eighty-six of these photographs were presented to one hundred reliable persons. Each photograph was numbered, and each subject received also three sheets of paper. Upon one sheet were numbers which corresponded to the numbers upon the photographs, and upon the other two sheets were merely a fairly complete list of names of emotions. This list of names was as follows:

Laughter
Smiling
Joy
Delight
Pleasure
Gladness
Glee
Happiness
Amusement
Bliss
Ecstasy
Cheerfulness
Rapture
Enthusiasm
Merriment
Astonishment
Amazement
Desire
Earnestness
Eagerness
Reverence
Religious
Friendliness
Pride
Haughtiness
Self-assertion
Calculation
Meditation
Reflection
Thought
Hate
Disgust
Contempt
Scorn

Wonder
Admiration
Surprise
Awe
Attention
Interest
Expectancy
Want of interest
Modesty
Humility
Self-abasement
Grief
Sorrow
Sadness
Despair
Mental suffering
Physical suffering
Pain
Displeasure
Annoyance
Irritation
Worry
Bore
Bitterness
Hardness
Love { Religious / Altruistic / Romantic / Sensuous
Pity
Sympathy
Vanity
Coquetry
Coyness
Liking
Tenderness
Longing
Yearning

Sneering
Loathing
Repugnance
Dislike
Aversion
Disdain
Antipathy
Rage
Fury
Anger
Distraction
Passion
Calmness
Resignation
Beauty
Ugliness
Fear
Terror
Horror
Suspicion
Dread
Alarm
Fright
Anxiety
Hopelessness
Despondency
Awe
Dismay
Timidity
Defiance
Determination
Firmness
Faith
Trust
Resolution
Aspiration
Relief
Hope

The following were the directions which the subjects received.

"*Experiment in Judgment of Expression.* Materials: Photographs, a list of words, and a list of numbers.

"1. Read through quickly the list of words in order to refresh your memory with the names of the different expressions. Observe each photograph carefully and write upon the sheet, opposite the corresponding number, the name of the expression which the photograph suggests to you. If one word

does not suffice to express the meaning, add the necessary words."

The subject was also requested to write down his introspection.

I present the facts in the case of twenty-four of the photographs. These facts are (1) reproductions of the photographs themselves, (2) a statement of the stimulus which led to the pose in each case, and (3) a statement of tabular form of the interpretations of each photograph by the hundred judges. This last statement is not complete, the names which occurred only once in the 2,400 judgments being omitted for brevity's sake. Their inclusion would make no appreciable difference, as they are few and are distributed between synonyms of an appropriate term and 'errors' in much the same proportions as the judgments recorded here.

The results give (1) means of deciding how far certain defined facial expressions are interpreted each as the sign of a given emotion or complex of emotions; (2) and, in cases where the facial expressions are clearly significant, means of studying emotional expressions and illustrating them before classes in psychology or dramatic art.

The photographs with their identification numbers are given in Plates 1, 2, 3 and 4.[1]

The stimuli to the poses were as follows:

3 was posed for the second line in Gretchen's speech to Faust:

"I feel it, you but spare my ignorance
To shame me, sir, you stoop thus low."

9 was posed for breathless interest.

11 was posed for attention to a purely intellectual matter. While mentally multiplying 19 × 19 the subject was photographed.

15 was posed for attention to an object.

18 was posed for suspicion. (This is more often interpreted by the hundred judges as *fear*. We may note that Bell describes fear and suspicion together. "In human fear and suspicion, the nostril is inflated, and the eye has that backward, jealous and timid character. . . ."

21 was posed for interest toward a child.

[1] The author will be glad to furnish series of the original photographs or half-tone reproductions thereof to anyone wishing to use them for research or instruction in psychology. The cost is not yet ascertained, but will probably not be over five cents apiece for the reproductions and twenty-five cents apiece for the originals in sets of twenty-four.

22 was posed for agreeable surprise.
29 was posed for pity ('Poor thing').
31 was posed for determination.
32 was posed for righteous anger.
33 was posed for horror.
38 was posed for physical pain.
44 was posed for fear, the exposure being made after the subject had said the word 'Poison' in reciting Juliet's speech in the potion scene—"What if it be a posion, which the friar subtly hath ministered, to have me dead?"
47 was posed for hate.
48 was posed for sympathy.
50 was posed for despair.
51 was posed for rage.
52 was posed for vanity.
55 was posed for disgust.
61 was posed for sneering.
62 was posed for contempt.
69 was posed for laughter.
77 was posed for religious feeling.
83 was posed for the first degree of suspicion.

The interpretations by the judges, omitting terms occurring only once in the 2,400 judgments, are given in Table I. Where the same photograph is described by one judge by two terms (*e. g.*, hatred and scorn), each is counted as one half. By following down any column, the interpretation of any one of the photographs may be seen clearly. For example, Photograph 61 has 93 judgments in the disgust, repugnance, sneering, scorn, contempt group, 1 of bitterness, 1 of defence, 1 of hate, 4 of the terms applied to it being omitted because occurring only once in the entire 2,400. By reading the table horizontally one sees what different expressions may be accepted as significant of 'modesty,' 'coyness,' 'fear,' etc. For example, coyness and coquetry, mentioned 66 times, are applied 16 times to No. 3, 16 times to No. 29, 28 times to No. 52, twice to Nos. 21 and 83, and once to Nos. 18 and 62.

It must be kept in mind that a part of the variation in the judgments of the same photograph is due to ignorance of the meanings of real facial expressions and to ignorance of the accepted meanings of the terms used.

It is hoped that the very considerable success of these posed photographs will lead others to publish snap-shots of men, women and children in naturally aroused emotional states. The last should be relatively easy to obtain.

TABLE I

Photograph	3	9	11	15	18	21	22	29	31	32	33	38	44	47	48	50	51	52	55	61	62	69	77	83
Modesty	22½	.	1	.	.	3	.	1	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.
Coyness	10	.	.	.	.	2	.	11	.	.	.	.	.	.	.	.	.	9	.	.	.	.	.	1
Coquetry	6	.	.	.	1	.	.	5	.	.	.	.	.	.	.	.	.	19	.	.	1	.	.	1
Contentment	2	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	1	.
Earnestness	2½	.	.	.	.	1	1	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.
Reproach	1	.	.	.	.	.	.	1	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.
Shyness	7	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.
Uncertainty	1	.	.	½	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.
Yearning	2	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	1	.
Disgust	.	.	.	.	2	.	.	.	.	.	½	2	.	12½	1	.	.	.	36	8	6	.	.	1
Repugnance	.	.	.	1	5	.	1	.	.	.	.	.	1	5	2	.	1	.	14	1	.	.	.	1
Dislike	.	.	1	.	1	.	.	.	1	4	.	.	.	2	5	.	.	.	7	2	3	.	.	1
Annoyance	.	.	1	1	3½	.	.	.	.	3	.	1	1	.	4	.	4	.	8	.	.	.	.	.
Loathing	.	.	.	.	2	.	.	.	.	.	.	.	.	2	.	.	.	.	4½	2½	.	.	.	1
Aversion	.	.	½	1	6	.	.	.	.	.	1	.	½	5	1	.	.	.	2	2	.	.	.	4
Bored	.	.	2	.	.	.	.	.	1	1	.	1	.	1	1	.	5	.	2	1	4	.	.	.
Sneering	.	.	.	.	.	.	.	.	.	.	.	.	.	2½	.	.	.	.	8	33	10½	.	.	.
Scorn	.	.	.	.	1½	.	.	.	2	.	.	.	.	7	2	.	1	.	3	16	11	.	.	3
Contempt	.	.	.	.	.	.	.	.	1	1	.	.	.	2½	.	.	1	.	3	19½	21	.	.	2
Disdain	1	.	.	.	.	.	.	.	.	.	.	.	.	8	.	.	1	.	1	7	21	.	.	1
Haughtiness	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	1	9½	.	.	.
Superiority	.	.	.	.	.	1	.	.	.	.	.	.	.	.	.	.	.	.	.	.	2	.	.	.
Snippy, superciliousness	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	2	.	.	.
Cynicism	.	.	1	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	1	.	.	.
Horror	.	2	.	.	3	.	.	.	.	.	35	.	9½	.	½	1	16½	.	.	.	.	.	.	.
Terror	.	.	.	.	1	.	.	.	.	.	13	.	14	.	.	5	.	.	.	.	.	.	.	.
Alarm	.	.	.	.	2½	.	.	.	.	3½	3	.	6	.	.	6½	2½	.	.	.	.	.	.	3
Fury	.	.	.	.	.	.	.	.	.	.	9	.	.	1	.	.	3	.	.	.	.	.	.	.
Frenzy	.	.	.	.	.	.	.	.	.	.	1	.	.	.	.	.	1½	.	.	.	.	.	.	.
Fright	.	.	.	.	2	.	.	.	1	.	7	.	12	.	.	2½	½	.	.	.	.	.	.	.
Passion	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	2	.	.	.	.	.	.	.

TABLE I.—*Continued*

Photograph	3	9	11	15	18	21	22	29	31	32	33	38	44	47	48	50	51	52	55	61	62	69	77	83
Suffering, physical					1						5	31½			3		3							
Suffering												3					½							
Suffering, mental			1						1	1		17	2	1	5	5	1							
Worry			1		2				3	8		8½	2		10	5			1					1
Anguish										1				2										
Despondent										1		2½				2								
Grief					1							1	2		1	3								
Pain											2	6	1	1		1	1		1					
Trouble										1		1												
Sorrow				1										½	3½									
Suspicion	2				6			1		2			1	1			1							31½
Antipathy					1					1			1	1	1⅓						1			2½
Distrust					½																			6
Jealousy									1															1½
Nothing			1							1	1													2
Timidity	1½					1								1										7
Watching				1																				2½
Doubt	1	1	1							1														1
Self-abasement					1																			1
Laughter									3													40		
Ecstasy																		3				11	1	
Glee																		1				17		
Merriment																						10		
Mirth																						2		
Rapture		1					1											2				6½		
Reverence																							25	
Religious																							15	
Religious love, faith, etc.																							25	
Aspiration		1																					1	

TABLE I.—*Continued*

Photograph	3	9	11	15	18	21	22	29	31	32	33	38	44	47	48	50	51	52	55	61	62	69	77	83
Adoration																		1					3	
Devotion																							4	
Humility	3																						3	
Prayer																							6	
Resignation			2																				2	
Suppliant																							2	
Trust				1														½					1	
Fear					18½					1	4		8½		2½	2								5
Anxiety			1	2	5					7		6	6		9½	4								
Dread					9					1	1	2½	5½	1	3	1	2							
Expectation of disaster					1					1														
Interest	4	1	15	22	1	23½	½			1								4						1
Eagerness			1	5	1																			
Enthusiasm		1		2			1															1		
Expectation		5	4	18½		2				2½			1					1						
Hope			2	5½			1			2													2½	
Want of interest	1		1	4	1	1						1			1	1								
Amusement	1					8½		½										2				2		
Cheerfulness			1			2																		
Friendliness						4½		2										2						
Liking	1			1		5		1																
Meditation	1		2			4						1				1								
Romantic love				1		2	1																1	2
Vanity						1		1										10			2			
Beauty																		5						
Bliss																		4						
Delight		1																5						
Gladness								1										2						
Happiness																		4				1		

TABLE I.—*Continued*

Photograph	3	9	11	15	18	21	22	29	31	32	33	38	44	47	48	50	51	52	55	61	62	69	77	83
Love								1										6						
Pleasure			1			2		½										8						
Playful							½	1										1						
Smiling				1		2												3						
Self-satisfaction																		2						
Desire		1		1												1		1						
Tenderness	2			1		10		18										1						
Sympathy	5		1	½		7½		14½		1			1		9½									
Pity	3			1		½		10						½	5									
Comforting a child								2																
Cajoling	2							4																
Chiding								2																
Loveal truistic, maternal						2		2																
Pleading								2																
Persuasion								2																
Soothing capability								2																
Attention	2	5	15	13		4½				2			1		½	1								
Calmness	1		9																					
Calculation			3	1		1			1	2					4						1			1½
Curiosity			1																					½
Reflection	1		5	1			½														1		1	1
Sadness	1		2																					
Deep, penetrating thought			2						1															
Amazement		13					9			2	4½		1			1	3							
Astonishment		13					10½	½		2	½		2½			2	1½							
Surprise		30				2	52	½		2	2		2½			2	2½					½		
Wonder		14	1	2½			12					1	1½			2								
Awe		6	1				2			2	1	1	2			1							1	
Admiration		4		1		1	3											1						

PLATE I.

3

9

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PLATE II.

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PLATE III.

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PLATE IV.

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77

83

TABLE I.—*Continued.*

Photograph	3	9	11	15	18	21	22	29	31	32	33	38	44	47	48	50	51	52	55	61	62	69	77	83
Determination			2						23½								½							
Assertion of will power									1								1		1					
Bitterness					3				5	1		2		2½						1				
Displeasure			1	½	2				8	4			1	1	5				4		1			
Defiance					3				4	1	1			5⅓			2½			1	1			2
Disapproval									2															
Firmness									8½	½							½							1
Hardness									3	2			1	2½	1		½							
Resolution									6	2														
Self-assertion									1					1										
Sternness									3															
Despair					½											13	1							1
Dismay					1		2			4	1½	1	3		1	8	4							
Distraction					1					1		1				10	1							
Bewilderment																2								
Hopelessness												1			1½	1		½						
Perplexity										2		1			1	3								
Racking brains																1½								
Thought	3		4	½						4						5								1
Anger					1				7½	9	1½		8	1⅓			9							1½
Hate					1½				1					8			3			1				
Hatred									½										1					
Indignation									2	1							2				1			
Dissatisfaction										1							1							
Hurt			1							1														
Irritation			1		1				1	4				2	5				3					
Rage					4				1		4		1				9							
Puzzled										2														
Ugliness														13										

A SLIT-MECHANISM FOR SELECTING THREE MEASURABLE MONOCHROMATIC BANDS

BY H. M. JOHNSON

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The device herein described was designed for the purpose of selecting a single band of spectral light, which is to be matched in hue by mixing two other bands. For demonstration-experiments it is desirable, and in precision-experiments necessary, that the wave-length of the stimulus-bands be accurately measurable. As the writer's purpose made the use of a single spectrometer system desirable, the method of selecting the bands presented some difficulties. It was necessary to obtain a spectrum of maximal purity and intensity: hence it was inadvisable to use a long slit in the collimator-tube or to use an objective lens of great focal length to increase the height of the spectrum. For this reason it was decided to select the three bands from the same horizontal plane.

Abney[1] devised a set of three slits, moved independently in a groove in which they fit. Each slit opening is made by two jaws which form the long sides of a parallelogram whose short sides are two pieces each of which is pivoted to a projection in the center-line of the slit-opening and fastened at either end to one of the jaws. A screw-and-spring mechanism regulates the width of the slit-opening. The method of selecting a given band is as follows: The width of a given slit-opening is found by comparison with a standard slit. The slit is then placed in the groove and moved by hand to the desired place. This is indicated on a transparent scale, calibrated in terms of wave-length, which is fastened above the groove. An image of this scale 10 times magnified is thrown on a screen; from which readings can be made to .02 mm. This device, though ingenious, and from Abney's account satisfactory, was not adopted because of its manifest clumsiness.

[1] Abney, Wm. de W., Researches in Color-vision, etc., N. Y., Longmans, 1913.

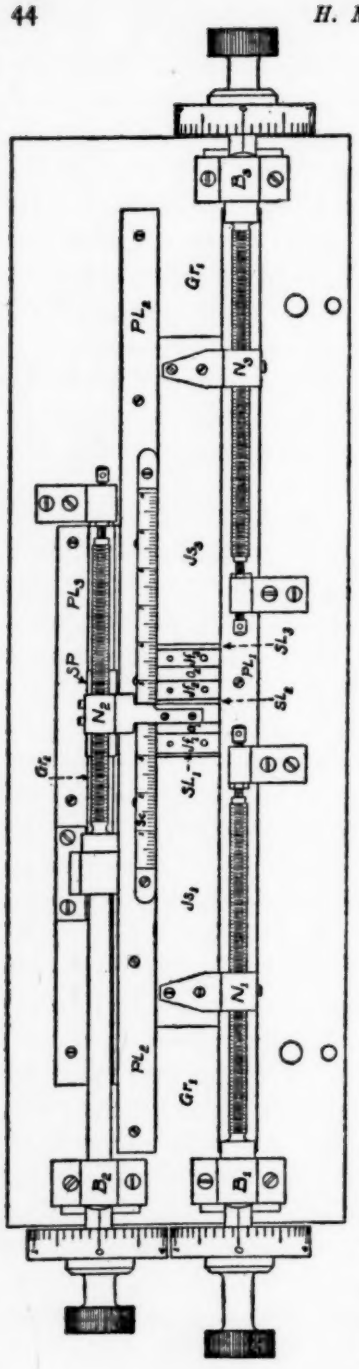
The double slit designed by Watson[1] and built by Gaertner and an earlier model of similar type—the Donders coupled slits, suggested this design in essential. The work was executed by Mr. William Würth, the mechanician of this laboratory, at a cost of about $90.00. It was found possible to make the wall and the shafts of the micrometer screws considerably shorter than in the Donders and the Watson models, and to use a single scale for the coarse readings. These changes tend toward compactness and simplicity without disadvantage.

The accompanying figure shows the construction in detail. All the parts are of brass except the shafts, nuts and bearings of the micrometer screws and the millimeter scale, which are of steel.

In the slit-wall a window is cut 1 cm. high and 9 cm. long. Its vertical limits are visible through the three slit-openings Sl_1, Sl_2, and Sl_3 and the intervening spaces O_1 and O_2 in the cut. Each slit-opening is formed by two knife-edged jaws, one free (Jf_1, Jf_2, Jf_3) and one (Js_1, Js_2, Js_3) attached to a split nut (N_1, N_2, N_3) working on a micrometer screw. All the jaws are beveled to fit accurately into a common groove, Gr_1, in which they slide. This groove is formed by two parallel beveled plates, Pl_1 and Pl_2, fastened by screws to the slit-wall above and below the slit-window, respectively. The free jaws Jf_1, Jf_2, Jf_3, are held in place by spring clips fastened to the center of the jaw on the reverse side and binding against the beveled edges of the window.

The greatest mechanical difficulty was the prevention of a serious amount of lost motion in jaw Js_2, occasioned by its narrowness and the great distance between its screw-shaft and the groove Gr_1. This problem was solved by attaching the nut N_2 to a long strip of brass beveled to slide in a groove Gr_2, planed parallel to groove Gr_1, and like the latter, formed by beveling two brass plates Pl_2 and Pl_3. By this means the knife-edge of the jaw Js_2 is held as truly normal to its groove Gr_1 as are the other jaws. For the suggestion which led to the adoption of this device the writer is indebted to Dr. P. W. Cobb.

[1] Yerkes, R. M. and Watson, John B., 'Methods of Studying Vision in Animals,' Behavior Monographs, Vol. 1, No. 2, N. Y., Henry Holt, 1911.



The three micrometer screw-shafts are threaded 0.5 mm. to the revolution. Each of the 50 divisions on the screw-head therefore reads 0.01 mm. If all the settings are made by moving the screw in the same direction, the readings are accurate to limits within this order of magnitude. In making a given setting, say for slit Sl_1, the free jaw Jf_1 is pushed to its proper place by the jaw Js_1, the latter is now withdrawn beyond its proper place and returned. The reading to 0.5 mm. is made on the scale *Sc* from the edge of the jaw; the finer readings are made on the head of the micrometer screw.

The greatest proximity of the two extreme bands which this device allows is 2 cm. plus an amount dependent on the width of the slit-openings. In the spectrometer system to which it is attached at present, the beam emerging from the collimator is passed through two carbon disulphide prisms and brought to a focus on the slit window by an objective lens of 15½″ focal length. With this arrangement, the prisms being set at the angle of minimum deviation for the sodium line, the linear separation of the red lithium line ($\lambda = .670\mu$) and

the sodium line ($\lambda = .589\mu$) is about 15 mm. The linear separation of the sodium line from the green strontium line ($\lambda = .548\mu$) is about 24 mm. Sufficient separation may be obtained in another way: by using a single dispersion prism with an objective lens of great focal length. Such a lens however is very expensive if it be accurately ground, and the loss of radiation occasioned by its use is greater than that made by the second prism, since the height as well as the length of the spectral slit-image is magnified.

The bands selected by this mechanism may be deflected to their projection lenses by right-angled prisms.

The apparatus can also be used as a single or double slit when required. When it is made a permanent part of a spectrometer system the scale should be calibrated in terms of wave-length. The method of calibration, which is quite simple, is described by both authors cited above. Selection of the bands desired can thus be made quickly and accurately.

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PSYCHOLOGY AS A SCIENCE OF BEHAVIOR

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To those who have grown suspicious of the definitions and methods commonly employed in psychology it is a most hopeful sign that this suspicion has gained active and vigorous support among psychologists themselves. There is evidence at present of a pronounced disposition to pause for a consideration of fundamentals. What is psychology anyway,—what is its subject-matter and what are its methods? The stock definition that it is concerned with 'the description and explanation of states of consciousness as such,' states of consciousness being something which everybody knows and nobody can define, has fallen or is falling into disrepute. Yet the assumptions involved in this definition and the procedure based on it have persisted. Criticism seems to have had no appreciable effect. Now, however, comes a challenge which cannot be ignored so lightly. This challenge comes from the *sanctum sanctorum* of the laboratory itself. It declares that the conceptions which prevail in psychology are inept for laboratory purposes. Introspection is a broken reed. All that is significant in psychology is retained and provided for if we regard psychology, not as the science of mental facts through the medium of introspection, but as a study of behavior.

This is the contention advanced by Professor Watson in a recent number of this REVIEW.[1] He charges roundly that "human psychology has failed to make good its claim as a natural science" (p. 176), adducing as evidence the futilities which pass at present as scientific psychology and the impossibility of terminating disputes concerning facts which are inaccessible to experiment and derive their warrant wholly from an esoteric method known as introspection. "I firmly believe that two hundred years from now, unless the intro-

[1] 'Psychology as the Behaviorist views it,' March, 1913, pp. 158–177.

spective method is discarded, psychology will still be divided on the question as to whether auditory sensations have the quality of 'extension,' whether intensity is an attribute which can be applied to color, whether there is a difference in 'texture' between image and sensation and upon many hundreds of others of like character" (p. 164). There is but one remedy for all this, viz., to change our problem. "What we need to do is to start work upon psychology, making *behavior*, not *consciousness*, the objective point of our attack" (pp. 175–6).

The point of view thus briefly indicated meets with considerable approval from Professor Angell,[1] who protests, however, that the indictment is too sweeping. While the procedure of the behaviorist is undeniably objective and scientific, it is at the same time subject to serious limitations. To confine ourselves to the study of behavior may be quite in place as long as our subject is a rat in a labyrinth or a young beaver in a third floor apartment. And it may be admitted, further, that the study of behavior is of great significance in human psychology. But it is also true that "what happens between the time a stimulus affects a peripheral organ and the later time at which some reaction is made, we can often only judge with approximate accuracy provided the individual concerned tells us what has passed in his mind during the interim. The same thing is true of those reactions which are made in seeming independence of any immediate sensorial excitation. In other words, we have not at present any technique for ascertaining the train of neural units intermediate between a specific sensorial stimulation and a specific delayed response. This gap we must bridge over with information gleaned from essentially introspective sources or else leave it open" (p. 266).

A further limitation of behaviorism lies in the fact that it arbitrarily excludes from psychology an interesting and legitimate field of investigation. There are persons "to whom mental process as mental process is the only fascinating and ultimately worthy subject of study." To leave out mental process is for such persons, to omit Hamlet from the plot. "To recognize and describe the *external expressions* of love,

[1] 'Behavior as a Category of Psychology,' this REVIEW, July, 1913, pp. 255–270.

hate, and anger is as different from the actual experience of these thrilling emotions and from the description of them as immediately felt, as is the inspection of a good meal from the consumption of the same. To such an one any abandonment of introspection must seem a pitiful and mean desertion of the real objects of worth. Whether this view permanently prevails or becomes an esoteric scientific cult, it is a safe prediction that we shall always have it with us" (p. 269).

The behaviorist's program, then, according to Professor Angell is inadequate, first, because it is frequently unable to trace out the behavior of the organism without appeal to the mental processes which are going on simultaneously, and, secondly, because the rejection of mental processes as worthy objects of study is an unwarranted proceeding. Introspection still has its rights. "Let us then bid the movement toward objective methods and objective description God-speed, but let us also counsel it to forego the excesses of youth" (p. 270).

It is worthy of note that the charges brought against introspection are by no means controverted in Professor Angell's article. " 'Tis true 'tis pity, and pity 'tis 'tis true." The defense consists mainly in showing that the behaviorist had better not be throwing stones so recklessly, since he is clearly in need of an ally. Theoretically, indeed, the attempt to get at the facts from the outside, through the medium of behavior, can go a long way, but practically it encounters difficulties early and often. To achieve without the aid of introspection an extended analysis of color experiences into simple qualities, or to ascertain the peculiarities and scope of perception, memory, and imagination is abstractly possible, no doubt, but as a matter of fact, such work must necessarily be schematic and crude. How are we to learn the peculiar *modus operandi* of memory processes, unless the subject reports facts such as the presence of visual or auditory images? The purely subjective facts of which introspection puts us in possession are valuable, both for their intrinsic interest and for their service as clues to behavior. Introspection, therefore, is too valuable a tool to be lightly thrown away.

There is room for the suspicion that this line of defense

combines two things which in the interests of clearness should be kept apart. It is, first of all, a plea for introspection. Professor Angell declines to "embark on the troubled waters of definition. Suffice it to say that, however introspection be defined and whatever merits and defects may be alleged to attach to it as a method for ascertaining facts, all, so far as I know, are agreed that we are directly cognizant of our own experience in a manner different from our indirect apprehension of the experience of others. Whatever this *direct* mode of approach may involve under final analysis, it may serve for the moment to represent the sort of thing I have in mind by introspection" (p. 268, note). So much for introspection. But at the same time it is clearly taken for granted that the things revealed by introspection are in the first instance 'purely subjective facts.' Hence the assumption is made that if the behaviorist finds it necessary or expedient to use facts obtained by this 'direct mode of approach,' he ceases, so far forth, to be a behaviorist and returns once more to the wallow of subjectivism from which he extricated himself so recently and with so much pain and effort.

The plausibility of the argument lies, it would seem, in the illicit union of the 'direct mode of approach,' here called introspection, with subjectivism. In order to put an end to the scandal, the two parties to the union must be forced either to dwell apart or else to live openly before all men in holy wedlock. The behaviorist is in a position to view both alternatives with equanimity. The fact that he makes use of the direct mode of approach to obtain data convicts him of disloyalty to the concept of behavior only if such approach be taken as equivalent to subjectivism. But why should it be so taken? It would be just as reasonable to charge an exceptionally keen-eyed scientist with being an introspectionist because he observes facts which his less gifted colleagues can reach only in a round-about way. Nay more, since introspection is identified with inspection, every mouth is stopped and all the world has become guilty before the tribunal of the introspectionist. Thus interpreted, however, introspection ceases to be significant as a distinctive method. The reproach—or

the glory—of the introspective method is that it deals with a unique subject-matter, by virtue of which fact it is esoteric in character. This is the substance of the accusation which is brought against it. The hypothetical scientist just alluded to would not suppose that he was making use of any distinct method, simply because he could see what others were unable to see. Nor is it obvious why the behaviorist who selects as his problem the behavior of our scientist in making this observation is any more of an introspectionist if he consults his subject in the gathering of his data.

It would seem, then, that if introspection is to mean simply a 'direct mode of approach' and nothing more, our question disappears. It is not the mode of approach but the assumed nature of the subject matter that has made objective verification impossible. Unless we postulate a distinct subject-matter, we have simply returned to the non-reflective and naive use of consciousness; and as Professor Watson says, "in this sense consciousness may be said to be the instrument or tool with which all scientists work" (p. 176).

If, however, we take the second alternative and assume that introspection has a subject-matter all its own, the cause of introspection profits quite as little by Professor Angell's argument. Granted that the study of the external expressions is not a study of the experience itself, what do we gain if we appeal to the industry which is taking to itself the vestments of an 'esoteric scientific cult'? Is it a description of the experience to say, in the language of Professor Watson, "this, as a whole, consists of gray sensation number 350, of such and such extent, occurring in conjunction with the sensation of cold of a certain intensity; one of pressure of a certain intensity and extent, and so on *ad infinitum*"? (p. 168). If the description of thrilling emotions be the goal, the dime novel can beat psychology at its own game. Professor Angell's defense of introspection suffers from the serious handicap that he has permitted the case of his client to go by default. A counter-attack on the prosecuting attorney, together with a eulogy on honesty and sobriety, can scarcely be held to prove that his client is innocent of the charge of being a rank impostor.

So far, then, the upshot of the matter seems to be, on the one hand, that behaviorism, while incontestably scientific, is not exactly psychology, and on the other hand, that the study of 'subjective facts' or 'mental states,' while it may be entitled to the name of psychology, is neither scientific nor descriptive. If we insist on science, we must take up the study of behavior; if we crave description, our best course is in the direction of the literature of fiction. Meanwhile psychology, such as it is, remains with us. Professor Angell takes for granted that we must recognize the existence of 'mental terms,' accessible only through introspection. That there are such mental facts or 'pure psychics,' Professor Watson does not undertake to deny. "I confess I do not know. The plans which I most favor for psychology lead practically to the ignoring of consciousness in the sense that that term is used by psychologists to-day. I have virtually denied that this realm of psychics is open to experimental investigation" (p. 175). There may be mental facts, and if so, they constitute a legitimate subject for study. Moreover this study can invoke etymology in behalf of its claim to the name of psychology; and it can cite history to prove that it is the legitimate descendant of what has previously passed under that name. The worthless character of the claimant, however well established, does not warrant the usurpation of his family name and title by a stranger. If the behaviorist permits his opponent to maintain such claims, his proper course, it seems, would be to evacuate the premises and set up an independent establishment.

Such an arrangement, unfortunately, promises no lasting peace. Sooner or later the novelist will discover that he is in possession of a field which has hitherto been neglected by the sciences, and we may then anticipate that the novelist who writes like a psychologist will claim that he is really a psychologist who writes like a novelist. And he will point to the thrills in his emotions as evidence that he, and not the anemic devotee of structuralism, is the real psychologist. There is no alternative, then, but to go through the musty records and establish, if we can, the identity of the heir apparent, beyond further cavil. Who or what are these rival claimants, psychism and

behaviorism? Professor Angell admits the difficulty of defining the psychic, at least by implication; while Professor Watson states that he does not 'wish to go further into the problem [of psychics] at present because it leads inevitably over into metaphysics' (p. 175). The metaphysician, naturally pleased to find that he has a mission in life, can scarcely be blamed if he construes this indirect recognition as an invitation to state his views.

The situation is complicated by the fact that the advocates of structuralism would doubtless assert our statement of the issue to be an artificial simplification of the case and hence merely a begging of the question. Would they admit that their subject-matter is the psychic, in the sense of an existence different in kind or 'texture,' so to speak, from material objects? Their utterances on this point leave room for doubt. While there is much talk of consciousness, mental states, and psychic processes, it is also contended—for example, by Professor Titchener[1]—that the subject-matter of the psychologist is the same as that of the physical sciences. "It is the same experience all through; physics and psychology deal with the same stuff, the same material; the sciences are separated simply—and sufficiently—by their point of view." Experience taken in its independent aspect is physics and chemistry; taken in its aspect of dependence on the body it is psychology.

This view seems to place itself beyond the reach of criticisms directed against the hypothesis of mental states or 'consciousness as such.' Psychology and physics do not deal with different materials. It is the standpoint, not the stuff or subject-matter, that differentiates them from each other. But what constitutes dependence on the body is not made very clear. My own labors on this point lead me to the conclusion that the word dependence conceals an ambiguity which makes it possible to interpret consciousness in terms of behavior or in terms of mental states, as occasion may require.

To illustrate the standpoint of psychology, Professor Titchener cites the difference between physical and psychological time. Physical time is constant, psychological time is

[1] 'A Text-book of Psychology,' Chapter I.

not. 'The hour that you spend in the waiting-room of a village station and the hour that you spend in watching an amusing play are physically equal; they measure alike in units of 1 sec. To you, the one hour goes slowly, the other quickly; they are not equal' (p. 7). Time from the one standpoint is subject-matter for physics; time from the other standpoint is subject-matter for psychology.

But what, more precisely, is the nature of this difference? One would hardly care to account for the interminable boredom of waiting for a train in an out-of-the-way place by saying that a psychical or apparent time comes into being and adds itself to the physical time and thus brings about the peculiar length of the wait. The time which I experience and which taxes my endurance is the only time there is. Nor can I hope to ascertain the 'real' length of that time by consulting some person with ideal fortitude of character in order to learn how long the time seems to him. His time is psychological time quite as much as mine. The only conclusion, then, which we can draw is that physical time is a certain measurement of this duration, a rendering of it in terms of another duration—swings of a pendulum, for example—in order to get an equivalence. It all depends on the kind of inquiry that I make concerning the time in question.

To this, if I interpret him correctly, Professor Titchener would agree. It should be noted, however, that unless we go beyond this point, the distinction between physical and psychological time in terms of dependence on the body is wholly inept. So far everything that has been introduced involves dependence on the body. The swings of the pendulum are as much an experiential fact as the boredom of waiting. The distinction, in other words, becomes a distinction in the kind of problem that we treat, with no pertinent reference whatever to dependence on the body. The physical problem has to do with mathematics and equivalences; the psychological problem concerns itself with factors such as attention, habit, preoccupation, etc. The duration is studied by the psychologist in relation to the activities of the organism, not in relation to a consciousness or with reference to its 'mental' constituents.

Dependence on the body means that the psychologist studies the time with reference to the behavior of the experiencing organism.

It is not long, however, before we come upon what seems to be a second meaning of the word dependence, and one which abundantly justifies the term. "Heat is a dance of molecules; light is a wave-motion of the ether; sound is a wave-motion of the air. The world of physics, in which these types of experience are considered as independent of the experiencing person, is neither warm nor cold, neither dark nor light, neither silent nor noisy. It is only when the experiences are considered as dependent upon some person that we have warmth and cold, blacks and whites and colors and grays, tones and hisses and thuds. And these things are subject-matter of psychology" (p. 8). Again we find the statement, "It is when heat-waves strike the skin, and sound-waves strike the ear, and light-waves strike the eye, that we have experience in its dependent aspect, as warmth and tone and color" (p. 10).

Statements such as the foregoing are no doubt open to more than one interpretation. In saying that the world of physics is neither warm nor cold, the writer may have meant simply that the physicist is not interested in these qualities, but chooses to confine himself to a study of motions. The relation of stimulus to sense-organ becomes prominent only when we turn to physiology and psychology. But if that is what is meant, dependence on the body is no criterion for the distinction between physics and psychology. The body is as much concerned in motions as it is in colors. We seem to meet in these passages what is in effect the old-time distinction between primary and secondary qualities. The distinction between physical and psychological is, it seems, no longer a distinction of problems; it has become a distinction of existence or order of being. With the advent of the experiencing organism, warmth and tone and color spring into being and add themselves to the sum-total of the things already existent in the universe. These qualities become dependent upon nervous processes in a manner that does not obtain in the case of motions. Dependence is now a dependence upon processes in the nervous system which characterizes some facts as con-

trasted with others. Thus it is stated that "our sorrow is the mental aspect of those nervous changes that make us cry: we have only to shift our point of view, and what appeared as nervous change appears as emotion" (p. 15).

As already indicated, the point that I wish to emphasize is that we must come to terms as to what is meant by dependence upon body. Taking the phrase in one sense, we find that it is simply a name for the difference in problems with which physics and psychology respectively are concerned. But taken in this sense the phrase becomes a misnomer, and suggests the propriety of reading off all our psychological facts in terms of behavior. Taking it in another sense, the phrase reinstates the distinction between primary and secondary qualities, in complete disregard of what history and logic have to say on this important subject. Taking it in the first sense we get a view of psychology which apparently turns it in the direction of a study of behavior. Taking it in the second sense we get nowhere at all. All experiences being dependent in the same sense upon nervous processes, psychology must necessarily oscillate, in much the same way as has sometimes happened to sociology, between the view that it is the only science and the view that it is merely a blanket term for all science, with no specific field or problem of its own. The plausibility of Professor Titchener's position, I am forced to believe, lies in taking both interpretations of dependence at once. In this way it is possible for him, on the one hand, to maintain connections with our common world and claim that the subject-matter of psychology is the same as that of physics. But on the other hand, he is also in a position to continue the tradition of a psychology which has, after all, an independent subject-matter and an esoteric method.

The elimination of ambiguity and the repudiation of mental states, however, does not justify the view that psychology is a study of behavior. Behavior is a wide term. Professor Angell suggests that "mental life, conscious process, as our psychologists have dealt with it, has had to do with reactions which were mainly concerned with new individualistic adaptations. The behavior which we should study in man would be,

in part, therefore, the old instinctive behavior, but in part this new personalisticly adaptive behavior" (p. 262). And he adds that "as a program this is entirely intelligible."

That this program is intelligible it would not be worth while at present to dispute. Whether it is valuable as a guide to investigation is a different question. It is not at all obvious why we need a new science to study this behavior, unless we find that this behavior is truly different in kind. What is the difference between instinctive behavior and 'personalisticly adaptive behavior'? From the standpoint of evolutionary history and inherited structure there is doubtless an important difference, but the distinctiveness of psychology must be based on a difference in the behavior itself, if the definition is to justify itself. The fact, however, that a given behavior is personalisticly adaptive behavior is not, apparently, revealed in the behavior as such. It is an extraneous character, and so can give no distinctiveness to the field of psychology. It would be quite as reasonable to subdivide the field of botany in the interests of a new science, and group together for separate botanical study those flowers which have enabled poets to give symbolic expression to the beauty of women.

The first difficulty, then, which we encounter is that of differentiating the behavior which is subject-matter for psychology from other forms of behavior. A further apparent difficulty springs from the fact that behavior presupposes relation to a stimulus. In the simpler forms of behavior stimulus and response may be correlated without practical difficulty. But when we deal with what Professor Watson elsewhere calls 'delayed overt response,' the matter becomes more complicated and the theoretical difficulty becomes more prominent. The behaviorist would not seriously undertake to record everything that happens between stimulus and response. He proceeds selectively, taking the relation of stimulus and response as his clue. He is properly interested in the movements which result from the application of the stimulus only in so far as they constitute response. Otherwise his study is not a study of behavior, but a study of movements. But when does a movement constitute a response? Do we

label as stimulus the spoken word which results in overt action a week later, or the visual perception which sets a complicated and long-drawn-out problem, for no other reason than that it appears somewhere as an antecedent in the causal chain of events? If so, there is no obvious reason why the event which occurred just before or immediately after the *soi-disant* stimulus should not be regarded as the true stimulus. Unless a satisfactory reason is forthcoming, it would seem better to substitute cause and effect for stimulus and response and to drop the term behavior from our vocabulary. Psychology then becomes a study of certain causal relationships, but is still without a principle for the selection of those causal events which are supposed to constitute its peculiar subject-matter.

Even if we manage to become reconciled to this situation, however, our troubles are not yet at an end. There still remains the difficulty in certain cases of showing that the event which is selected as stimulus or cause bears any significant relationship to the event which figures in our scheme as the response. The stimulus is supposed to have a causal connection with the response, but how are we to know that this is the fact? How are we to know that the engineer who solves a problem for me at my request might not have done so anyway? No behaviorist can possibly show that the airwaves set in motion by my vocalization were an indispensable stimulus. We doubtless believe that the spoken word was in fact the spark which lit the fuse and finally exploded the mine, but this belief involves a complication of causes which it is wholly beyond our power to control or to verify.

It is true, of course, that we are able, as a matter of fact, to correlate stimulus and response. I know that it was the spoken order which caused the commission to be executed, for the expert reminds me of the fact and presents a bill. But neither of us makes any pretense that his belief is derived from a scrutiny of the causal sequence. Memory furnishes us with a short-cut to the result. While our present acts are doubtless connected with the past through causation, we do not regard them as simply the effects of antecedent causes. They are rather responses to present stimuli. The expert presents his

bill, being moved thereto by a stimulus which may be indicated by saying that it is the spoken-word-constituting-a-commission-now-completed. That is, the stimulus cannot be pushed back and anchored at a fixed point in the past, but is a present factor at the moment of response.

The point that I wish to make is that if psychology is to be regarded as a study of behavior, we are bound to reinterpret the category of behavior. We are inevitably forced to consider once again the difference between acts that are instinctive or purely automatic and conscious acts. If we attempt to account for this difference by the doctrine that sensations or some other simon-pure psychic existences come into being, we raise more difficulties than we solve. This doctrine seems as unnecessary as it is incoherent, since it is possible to assign to psychology a type of behavior which is different in kind from other behavior. A purely instinctive response to a light-stimulus may properly be viewed as response to ether-vibration or wave-length. But if this stimulus results in what is commonly called consciousness, a different kind of response ensues. The light-stimulus becomes a cause or occasion for the act of looking. But why look, unless it be to secure a new stimulus for further response? We stop to look, precisely because the first response does not run smoothly off the reel. The response will not go forward, so to speak, but is halted and expends itself in the effort to secure a further stimulus. We have here a highly peculiar form of response, in that it is a response which seeks and maintains the stimulus necessary for further response.

We reach the same result if we say that conscious response is a process of organizing or readjusting different simultaneous responses which interfere with one another. Hence the pause during which the organism prepares for the final adaptive response in which the conflicting partial responses are harmonized. This is the moment of attention, of looking, which furnishes the organism with a visual object by which the further behavior is controlled. The stimulus and the response during this period of hesitation are correlative in the sense that the process of establishing a harmonizing adjustment for the conflicting responses is paralleled in the process by which

the visual object finally attains the status of an adequate stimulus.

From this standpoint the characteristic trait of conscious behavior lies in the fact that stimulus and response develop concomitantly. As long as the response is uncertain, the stimulus is likewise uncertain. The response which involves a series of steps has as its correlate a stimulus which provides for its own successor. In a response to a visual stimulus, for example, the stimulus proves to be a stimulus, not only for various further acts, but also, more specifically, for the act of further looking. The response which by repetition becomes habitual has a stimulus which is gravitating steadily in the direction of a purely physical cause. When the response is wholly unconscious, the stimulus is a stimulus only in the sense that it is a link in a causal sequence. It is no longer a stimulus in the sense which links up stimulus and response in a correlative whole, within which the constituents of the whole undergo progressive and concomitant modification.

It is perhaps unnecessary to say that this interpretation of the relation between stimulus and response is the doctrine set forth by Professor Dewey in his article on 'The Reflex Arc Concept in Psychology.'[1] The brief comments on the doctrine in the foregoing paragraphs are intended mainly to indicate the direction from which, in the view of the writer, the correct interpretation of psychology is to come. If we place ourselves at this standpoint, we are in a position to accept the contention that psychology is a science which has to do with behavior. We get rid of the obscurities and ambiguities which are inherent in the current conceptions of sensations and images and of mental states generally; and at the same time we guard against the danger of taking behavior in a sense which permits the distinctive and significant trait of conscious behavior to disappear from view. In giving proper recognition to the peculiar character of the stimulus, we are led to interpret the current doctrines of such processes as attention, association, imagination and memory as simply formulations of the changes which stimuli undergo in their function of controlling response. In

[1] This REVIEW, 1896.

shifting these processes from 'consciousness' to things, we lay down an intolerable burden of mystery and contradiction. These processes no longer appear as independent facts, to be analyzed back somehow into pre-existent elements, but are interpreted solely with reference to the behavior with which they are correlated and to which they ordinarily furnish the most direct clue. While it is entirely legitimate and frequently necessary to emphasize response rather than stimulus, the proper goal of all psychology is to give a description of behavior in so far as it is determined by this unique form of control.

Psychology as thus understood is not open to the criticisms that are urged against current introspectionism. It does not call for an attempt to 'reconstitute' the experience of the subject, but rather to ascertain whether and in what specific manner stimuli exercise this peculiar type of control. As to introspection, its distinctive trait is neither its character as a method, nor the nature of its subject matter, but its problem or aim. I have contended in this paper that in conscious behavior the stimulus undergoes what Professor Dewey calls a process of reconstitution, the goal of which is a stimulus capable of evoking a final response in which the confusion of the several partial responses is disentangled and harmonized. The peculiarity of introspection seems to consist in taking a stimulus which has been thus reconstituted so as to be effective for such response and treating it as a candidate for a different process of reconstitution, the purpose of this latter process being to discover the physical and physiological conditions which are involved. This happens, for example, when we interrupt the drinking of tea in order to analyze the taste. As a result of this manœuvre, the taste undergoes a peculiar change,[1] so that a scent and a temperature appear. This fact is not to be taken as evidence that these sensations existed as primordial psychic constituents of the original stimulus, but that the sense-organs thus indicated help to determine the response in the drinking of tea. And similarly the discovery of overtones, of brightness and intensity, and other facts per-

[1] For a more extended discussion of this type of change I may refer to a former article, 'The Method of Introspection,' *Jour. of Phil., Psych. and Sc. Methods*, Feb. 13, 1913.

taining to the stimulus that is analyzed are not to be read back as sensations or their attributes, but should be taken as clues to the presence of certain physical factors. This same analysis of the stimulus is physics, if we drop the reference to the behavior of an adaptive organism.

In conclusion it may be pointed out that the procedure involved in this standpoint is necessarily of an objective and experimental character. The behavior which is studied is the behavior of an organism; the factors which are sought are physical and physiological in character. It is only by further progress in the direction of objective methods and objective description that psychology can free itself of the reproach which is heaped upon it by members of its own household and take the place which rightfully belongs to it in the community of the sciences.

THE SELF AND THE EGO[1]

BY KNIGHT DUNLAP

I have been asked to make clearer my distinction between the 'Self' and the 'Ego,' and I think I can do so by explaining more fully my fundamental psychological postulates.

My conception of the empirical self and my further notion of the Ego as an essential presupposition of psychology, which I have set forth in Chapters XVI. and XX. of "A System of Psychology" are organic parts of a general view which discards completely the psychophysical theory of Descartes, which theory, or rather Malebranche's amplification thereof, has become so firmly embedded in modern thought that it is usually accepted without question by psychology, science and common sense. It is necessary that I make this general statement, for I admit without hesitation that the attempt to put my notions concerning the Ego into orthodox Cartesian terms is useless.

In the first place, I postulate the reality of the sensible world. Those who choose to postulate its unreality are free to do so, just as those who postulate the proposition that two straight lines may be drawn through a given point parallel to a given line are free to do so. As there is built up on this postulate of parallel lines an entirely coherent system of geometry, so on the postulate that the world of sensible things is unreal and that reality lies in the world of non-sensible matter, an entirely coherent system of psychology might be constructed. I do not think it has yet been done, however.

In the second place, I insist on a strict use of the term *consciousness* (or its equivalents), and the discrimination of the two ways in which that term is currently used. If we use it for the awareness, or being aware of, a datum we have no

[1] A contribution to a symposium before the Southern Society for Philosophy and Psychology, at The Johns Hopkins University, April 9, 1913.

business to use it also for the datum itself (say a sensible datum or color, which for exactness we may call a *sentiendum*[1]). If we do use it promiscuously in both ways, we introduce into psychology the same lack of significance which would be introduced into a lease by using 'lessee' and 'lessor' interchangeably in the same document.

It is curious that psychologists in general seem to have been bent on the ignoring of the distinction between consciousness and its object. James recognized the distinction very clearly, but buried the evidence so deep in his *magnum opus* that most of us who were brought up on James never had a gleam of it, but were taught that the 'state of consciousness' was both the awareness of the red, and the red color itself, whereas it was, for James, neither.

The distinction between the content and the consciousness of it: between the *sentiendum* and the *sentiens*, may perhaps be made clear by supposing for the moment that a color exists whether anyone is aware of it or not. There is, let us say, a spot of red on the wall. I may see that red spot and attend to the *red*. It is the *red itself* of which I am speaking now, not the ether vibration. I never perceive directly the ether vibration. I may never have heard that there are such things. But I see the *red*. Now, let me close my eyes; let everyone else in the room close their eyes; let God close His eyes. Let us now all think of things other than red: of the high cost of living; or one of Glück's operas, for example. Assume that the red is unchanged, as for all we know, it may indeed be. Now the red, still existing but unperceived, is the sentiendum, the potential content. When I again open my eyes and turn my head in the proper direction, the red goes right on existing, but awareness of the red is now added. The red is unchanged, but becomes a 'content of my consciousness.' There are then two factors to be reckoned with: the red and my awareness of it.

The distinction holds, while red is a content, no matter whether the red exists 'out of consciousness' or not. The supposition that red exists when not perceived brings out

[1] See 'The Nature of Perceived Relation,' PSYCHOLOGICAL REVIEW, 1912, XVI., p. 416.

sharply the distinction between the red and the perception of it, but the truth or falsity of the supposition has no effect on the situation while red is being perceived. It is as if we had before us a quiet, smooth-shaven man. By assuming that when he grows a beard, he will still be quiet, I may bring out the distinction between quietness and beardlessness, although the actual fact may be that when he grows whiskers, he becomes noisy and violent.

Once recognized, the distinction between the content and the consciousness simplifies the business of the psychologist immediately and immensely. For one thing, the elaborate doctrine of copy-images, with their many contradictory features, may be thrown overboard at once.[1] And I may remark in an aside that I believe that if this dead weight be jettisoned, psychology may escape the utter shipwreck in which, in the estimation of all non-psychologizing scientists and the estimation of many psychologists also, it seems now to be tossing.

We have, therefore, two problems before us. First, the analysis of the objects of content, in the attempt to discover the number of elements and their peculiarities; the study of the processes in content; and the study of the conditions (physical, in the broad sense) under which the various forms and elements appear. Parenthetically I may remark that the lions which have long stood in the way here are seen to have neither teeth nor claws, when we recognize that we are not dealing directly with consciousness. The often repeated statement that however we may analyze the concrete experience into elements, the experience was just as unitary as the elements, is seen to mean merely that whereas the content of an experience may be complex, the consciousness or experience of that content is not complex; or rather, that we do not analyze it.

In the second place we have the study of the conditions of consciousness. We can never directly observe consciousness, since consciousness is always the observation of a content.

[1] On the question of 'imagery' I shall, at the suggestion of one of the most discriminating of the reviewers of my book, attempt to make myself clear in a separate article.

We can, and must, however, examine the physiological conditions of consciousness; and also its logical conditions.

Consciousness, we find, has degrees. I am attentive or less attentive; the content is high or low in vividness. These things are partially synonymous. 'Consciousness,' 'attention' and 'vividness' are ways of expressing the fact of awareness in various degrees. To this I will return in a moment.

We may also be conscious in different ways, irrespective of differences in vividness. We may be conscious of content which is said to be 'present'; this consciousness I prefer to call by the old name *intuition.* Or we may be conscious of content not present. This we call *imagination.* Please take notice that these statements are not definitions. 'Present' content means merely 'intuited content' and 'not present' content means merely 'not intuited' content or 'imagined' content.

Other ways of being conscious are distinguished, the classification having possibly (but not certainly) no reference to the two classifications just mentioned. Consciousness of certain sorts of content is called *sensory* apprehension or *sensory* intuition: sometimes it is called *sensation:* but the term 'sensation' is more frequently used for the 'sentiendum.' The confusion is here so great that we would do well to exclude the term sensation from scientific discourse. Consciousness of certain other sorts of content is called *intellect*, popularly at least, and should be so called scientifically. Whether we are to apply the term 'feeling' to a way of being conscious, or to the affective content, is yet to be decided. At present it is applied equally to both.

Another division is between *introspection* and whatever is left over from that; non-introspective observation. Although I have no use for the orthodox *theory* of introspection, I propose to use the term, and to define it and use it as it is actually used by modern psychologists; namely, to signify the observation of kinesthetic and somatic sensations, and of affective contents. The only difference between my usage of the term and the usage of psychologists in general is that I explicitly define it as I use it, and that others sometimes, but not uni-

formly, include under it the analysis of the ideal content of imagination (so-called 'images').

Now I have outlined a simple system of psychology which covers all the ground in a way which seems to me to offer greater possibilities in the way of research than does the Cartesian method. I have provided for the recognition of a Self, which is basally the body, as constituted by the kinesthetic and somatic sensations, and by the 'feelings.' *Introspection* is rightly called *self-observation.* Emotions, since they are fundamentally complexes of bodily sensations and feelings, are rightly said by philosophers of all ages to be modifications or passions, of the Self. This, of course, does not exhaust the Self, but merely specifies the essential basis. The relations of this constantly experienced, and in itself relatively constant basis, to the more variable objects of 'external experience,' are also important. I am not at present interested in delimiting the Self; I do not know that it is possible to show where the Self leaves off and the non-Self begins. I am merely interested in identifying the concrete basis, or I might say, the core of the Self. I might go on another tack and show the importance of this Self, or rather of the rhythmic changes which are inherent in it, in determining our thought processes, but that is also out of the present line of interest.

I have now covered by title all the ground which can be studied, but I have also definitely involved something (if I may beg the use of the word *thing* in a rather vague sense), which cannot be studied, although the involution of the 'thing' cannot be escaped.

There is more to be considered than the content and the consciousness. Each awareness has a reference other than to the content. This may be made clear in the following way. Suppose three items of content, which we may designate as *a*, *b*, and *c*. Suppose I am aware of *a*, then of *b*, and then of *c*. The situation is quite different from that in which I am aware of *a*, another person aware of *b*, and a third person aware of *c*. This is so, even if we neglect the differences in *a*, *b* and *c* as they appear to three different persons. Neither is it true

that the awareness of *a*, *b* and *c* by myself establishes, or is the establishment of, a specific relation among the three. Assuming the existence of the three, there is no relation among them after I have perceived them that did not exist before I perceived them. This is demonstrable through the fact that my perception of the three items is not dependent on the perception of any particular relation among them. *a* may be brighter than *b*, and *b* than *c*, but I may successively perceive the three and not notice these relations at all. This proposition is true for any other relation that can be named. It is clearly not plausible that some relation, not specified, can account for the fact that I perceive all three.

The fact that I perceive all three, twist it as we may, remains to the end an ultimate, utterly inexplicable fact. The important thing about the situation that we are supposing is that they, the three items,—*a*, *b* and *c*,—are perceived by the same I. The perceptions are not the same; they may be temporarily separated by considerable intervals. What is the identity? Merely the identity of the I, the Ego. Manifestly, that which is identical in the case of the three perceptions is not that which is not identical. There is, therefore, the Ego, which must be discriminated from the awareness, perceptions or experiences which refer in common to the Ego.

Experience, consciousness, or whatever we may call that 'knowing' which James calls 'the most mysterious thing in the world,'[1] is quite clearly a polarized affair. At one end it refers logically to that which is known; at the other, to that which knows. Yet it is not in strict accuracy to be called a relation between the two, for a relation, as it is elsewhere understood, is between two objects, or knowable items, thus involving the possibility of this very consciousness of which we are speaking. We must be satisfied with considering consciousness as absolutely unique and irresolvable; there is nothing else in its class. We may call it an act, a relation, a quality, a process, a machine, or anything else, if we understand that these terms do not really describe it; for all of these terms refer primarily to objectivity and are intelligible only because we refer them

[1] James, 'Principles of Psychology,' I., p. 216.

to definite objective facts, and hence in applying them to consciousness we treat it as if it were objective.

There are thus three sorts of things which are immediately postulated as soon as we begin to talk of experience. *First*, the items which are experienced. *Second*, the experiencing of these items. You can no more talk about the objects without postulating the experience of these objects than you can talk about experience without postulating something which is experienced. *Third*, in postulating the object, or in postulating the experience of the object, you postulate that which experiences. We cannot talk of experiencing without an I which experiences; I can talk of the experience which I 'have,' but not of any which I don't 'have' or which is not exactly analogous to that which I 'have.' And it is certainly true that I 'have' no experience which I don't 'have.' We might suppose the existence of uncoördinated experiences, but these are then by hypothesis experiences which I do not have and which no other Ego 'has,' and hence are not to be considered as anything more than hypothetical. In other words, all experiences which are other than merely hypothetical, intrinsically postulate the I.

But what have we postulated when we have postulated the Ego? Nothing more, so far, than a certain coördination of experiences. There is one coördination of experiences which is expressed by saying that two experiences may be of the same object. It may be held that such a coördination does not exist: that no two experiences can be of the same object: this proposition is, however, itself an arbitrary postulate, a postulate, that is, which is in no wise necessary. We can avoid both this and the contrary postulation by saying that *if* two experiences are of the same object, there is *ipso facto* a correlation of experiences, and we may call that the *objective* correlation. Now all we have been claiming is expressed by saying that in addition to this possible objective correlation of experiences, there is an undoubted and unavoidable correlation of another kind, which we may conveniently call the *subjective* correlation, thereby fixing explicitly our use of the term subjective. This correlation is one of identity, and just as we

name the identity in the case of the objective correlation the *object*, we name the identity in the case of the subjective correlation the *subject*, or *Ego*. The object can be observed, and hence we can say more about it than merely to name it. The subject, however, is not observable, but is that which observes, and hence we can do nothing but name it, which we do by calling it the subject, the Ego, or that which observes.

It may be objected that the subject really is more than the observer; that it feels and wills as well as knows. This objection cannot be maintained. What we loosely call 'to feel' is really to observe or experience an affective content or feeling. To will is to experience a particular thing, namely, a feeling of desire or repugnance, in connection with some other content. The sole function of the Ego reduces absolutely to that of knowing, or, in other words, the sole subjective correlation is the correlation of experience.

DISCUSSION

THE PHENOMENA OF INDIRECT COLOR VISION

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I. My attention has been called to a recent thesis from the Bryn Mawr laboratory[1] in which the work of previous investigators of the color sensitivity of the peripheral retina is subjected to a vehement criticism.

Dr. Rand's attack deals chiefly with the question of the intensity of visual stimuli; and her arraignment of her predecessors is based almost exclusively upon her unique definition of intensity,—a definition which I believe no other investigator of the psychology of vision, with the exception of Dr. Ferree, would be willing to accept or even to consider seriously. Dr. Rand states that "intensity of stimulus will be used (throughout her paper) to indicate the *energy of light-waves* coming to the eye. Intensity of sensation, or apparent intensity, will be used as its correlative subjective term. So used, it will signify merely *energy or voluminousness of sensation and will have no reference whatever to the white-value of a color* (italics mine) . . . and the terms brightness and white-value will be used interchangeably to indicate the lightness or darkness of a color" (p. 20).

Starting out from these remarkable definitions she, naturally enough, reaches an equally remarkable result,—namely, that it is possible to 'standardize' visual stimuli by simply measuring their temperatures.[2] Armed with these two weapons,

[1] Gertrude Rand, 'The Factors that Influence the Sensitivity of the Retina to Color: A Quantitative Study and Methods of Standardizing,' *Psychological Monographs*, 1913, 15 (Whole No. 62), 166 pp.

[2] This method of determining the temperature (or the physical energy) of a beam of light did not, of course, originate in the Bryn Mawr laboratory. The method was described some twenty-five years ago by Langley, who pointed out however, even at that early date, that the range of its applicability is exceedingly limited; for in 1888 Langley warned his fellow physicists against the error of supposing 'that the luminosity

—her definitions, and her method of 'standardization,'—she engages in a merry tilt with her predecessors; and she endeavors to show that previous investigations of peripheral color vision have been, for the most part, a pathetic succession of misdirected efforts and ludicrous blunders.

Dr. Rand informs us, for instance, that Hegg's 'method of equating (the intensities of color stimuli) cannot warrant any conclusion concerning the relative limits of peripheral sensitivity to the different colors' (p. 32); and that Bull's 'method is an anomaly, and so far as the present writer knows is not justified in any investigation of color sensitivity that has yet been proposed' (p. 26). She alleges that Hess's 'object was primarily to furnish Hering with experimental evidence that would enable him to refute the Young-Helmholtz theory' (p. 26); Hess's 'assumption begins with a fallacy . . . (and) the assumption is itself incorrect' (p. 29). His test 'was both incomplete and wrongly devised' (p. 52); he 'used' his method 'very inadequately' (p. 40); he 'did not use the proper conditions of brightness of screen' (p. 50); and 'his test was thus again rendered unfair by his lack of proper conditions' (p. 50). As for Fernald's investigation, 'one may express surprise that work so sketchy should be considered as warranting any conclusion whatever' (p. 33). And Thompson and Gordon are found to have sinned with Fernald in employing a 'crude method' which 'lacks the first essential of standardization' (p. 60). And the present writer has apparently been guilty of every sin in the calender of science. My 'conclusion is apparently based upon a loose construction put upon the meaning of certain terms' (p. 13). "Like his predecessors, Baird also concludes beyond what is justified by his method of working" (p. 32). My blunders are due to 'failure to read carefully' (p. 53); and I have been guilty of misinterpretation in certain instances, and of mis-

of a color is proportionate to the energy which produces it.' He estimated 'that the same amount of energy may produce at least 100,000 times the *visual effect* in one color of the spectrum that it does in another.' (Italics mine.) (S. P. Langley, Energy and Vision, *Amer. Jour. Science*, 1888, 3d series, **36**, 359–379; see also *Phil. Mag.*, 1889, 5th series, **27**, 1–23.) From the foregoing one may see how inadmissible it is to employ such a method for the 'standardization' of visual stimuli.

representation in certain other instances. Ferree and Kirschmann, however, are among the favored few who escape Dr. Rand's general condemnation.

The criticism which is here urged against my work cannot be evaluated without a résumé of the situation. In a paper published in 1905[1] I mentioned the familiar fact that a 'color zone' upon the retina is found to be more widely extended in a peripheral direction when the stimulus employed in the exploration is more intensive;[2] and I also mentioned the consequence which obviously follows from this obvious fact,—namely, that no determination of the relative widths of color zones can furnish results which are really comparable with one another unless the stimuli employed have first been equated in brightness. A survey of the literature showed that Aubert had been the first investigator to realize the necessity of equating the intensities of his stimuli; that the same principle had subsequently been advocated by Chodin, Bull, and Hess; and that Landolt, Abney, and others had also shown that width of 'color zone' varied with intensity of stimulus.

Dr. Rand ascribes the discovery of this principle to Bull (1881); and she endeavors to show that I have erred in my statement of the views of Aubert (1865), Landolt (1872), Chodin (1877), Abney (1898) and others.

(*a*) In the case of Aubert, Dr. Rand directs her criticism against the fifth of the six paragraphs in which I summarized the findings and the conclusions of this investigator. She apparently sets out with the assumption that a one-to-one correspondence should somehow obtain between the several paragraphs in Aubert's statement of his summary, and the several paragraphs in my statement; nor is she daunted by the fact that the number of paragraphs is not identical in the two instances. She proceeds to compare one of my paragraphs, selected by herself, with one of Aubert's paragraphs, also selected by herself; and, naturally enough, she finds that the

[1] J. W. Baird, 'The Color Sensitivity of the Peripheral Retina,' Washington, D. C. Published by the Carnegie Institution of Washington, 1905, 80 pp.

[2] Various other factors which affect the width of the color zone were also mentioned and discussed; but these need not be considered here.

two paragraphs are not identical in meaning. She next alleges that when Aubert employs the term *Helligkeit* without a qualifying phrase he 'commonly refers to the intensity or brightness of the general illumination' (p. 16), and not to the intensity or brightness of the stimulus itself. From all of this it is to be inferred that I have mistranslated a paragraph in Aubert's summary, and that I have misinterpreted the meaning of *Helligkeit.* Dr. Rand concludes this remarkable argument with the statement that she 'is compelled to say that in a careful reading of all the articles by Aubert contained in the long list to which Baird refers, she is unable to find a single statement that would justify the conclusion that Baird has drawn' (pp. 16 f.).

In her further reading of the literature, however, she makes the disquieting discovery that both Bull and Hegg agree with my interpretation of Aubert; but instead of now withdrawing her criticism of my statement she counters with the additional charge that I have at any rate failed to give the page reference to the passage in question (p. 44); and she later repeats that 'Aubert does not in the references given by Baird' 'claim that brightness difference affects the sensitivity of the retina to color' (p. 52).

In reply to Dr. Rand's criticism it need only be mentioned that Aubert's 'Physiologie der Netzhaut' contains the following passages: "Denn dieselben Betrachtungen, die wir in §55 bei Gelegenheit des directen Sehens über die Helligkeitsverhältnisse der Pigmente und deren Einfluss auf die Wahrnehmbarkeit der Farben angestellt haben, finden auch hier Anwendung, so dass nicht zu eruiren ist, wie weit die Farben an und für sich Differenzen setzen. Wir werden daher nur sagen können: Contrast und Helligkeit der Farben sind von grossem Einfluss auf die qualitative Farbenempfindung, so wie auf die Grösse der Netzhautparthie, innerhalb welcher die Farben empfunden werden können" (p. 122). Section 55 to which Aubert here refers back contains the following statement: "Die Bestimmungen der Tabelle XV. sind also gewissermassen nur Brutto-Bestimmungen der Empfindlichkeit für Farben; zu einer Netto-Bestimmung müsste der Einfluss der

Helligkeiten eliminirt werden können, also *Pigmente von gleicher Farbenintensität* und Nüance auf einer Umgebung von derselben Helligkeit wie die Pigmente beobachtet werden. (Italics mine.) Solche Pigmente giebt es aber nicht, und da auch der photometrische Werth der prismatischen Farbentöne unbekannt ist, so erschient eine exacte Bestimmung des Gesichtswinkels, unter welchem die Farben empfunden werden können, überhaupt unausführbar" (p. 112).[1]

(*b*) Dr. Rand's criticism of my statement regarding Chodin is equally erroneous. She asserts that "Baird writes: 'Chodin remarks in his introduction: "It is self-evident that in comparing the retinal sensitivity to different colors, the color-stimuli employed must be of equal brightness and of equal saturation." But this very essential condition was not fulfilled in his own experiments' (see Baird, p. 20). Baird has here again made a misinterpretation. The rather free translation of Chodin's statement: 'Es bleibt nur übrig die Farben bei gleicher Sättigung und bei mittlerer Lichtintensität zu vergleichen' and the failure to read carefully the discussion following it are responsible, we presume, for the misinterpretation" (*loc. cit.*, p. 53).

A comparison of Chodin's statement with my statement (both of which are here appended in parallel columns) will show that Dr. Rand's charges of 'rather free translation' and of 'failure to read carefully' are wholly groundless.[2]

[1] The fifth paragraph of my summary reads as follows: "The relative extension of the different color zones cannot be determined with any degree of accuracy. Since the width of the color zone is a function of the luminosity of the stimulus, the color-stimuli employed in the determination of comparative retinal limits must all be equated in brightness. In the opinion of Aubert, the comparison of the relative brightness of stimuli of different colors is attended by such difficulties as to render its accurate accomplishment impossible, etc." The reader will note that that part of Aubert's statement which here deals with the effect of background is summarized in another paragraph of my paper (§1, p. 12). With regard to my alleged failure to include the foregoing passages in my published references to Aubert, it may be added that my paper contains a detailed reference to the page in Aubert upon which the first of the above citations is to be found; and this first citation itself specifically refers back to §55 wherein is to be found the second citation.

[2] Here as in the case of Aubert, my critic bases her charge upon her failure to find an identity of meaning in non-corresponding sentences which she has selected, apparently at random, from papers by these authors and from my monograph. In the present instance she has not even confined her selection to the page in Chodin which

Chodin's statement is as follows:

"Es ist selbstverständlich dass bei Vergleichung der Empfindlichkeit für verschiedene Farben diese letzteren von gleicher Sättigung und gleicher Intensität sein müssen." (A. Chodin, Ueber die Empfindlichkeit für Farben in der Peripherie der Netzhaut. *Archiv für Ophthalmologie*, 1877, 23 (3), p. 178.)

My statement is as follows:

"Chodin remarks in his introduction: 'It is self-evident that in comparing the retinal sensitivity to different colors, the color-stimuli employed must be of equal brightness and of equal saturation.'" (J. W. Baird, *The Color Sensitivity of the Peripheral Retina*, Washington, D. C., 1905, p. 20.)

In view of this statement by Chodin, in 1877, and of a similar statement by Aubert, in 1865, it is difficult to understand what justification Dr. Rand is able to find for her assertion (p. 25) that 'the first to recognize the need for making any sort of intensity equation of the stimuli used to investigate the relative sensitivity of the retina to the different colors was Ole Bull' (1881).

(*c*) Dr. Rand is in error again in her criticism of my discussion of the work of Landolt and Abney. She states that "Landolt and Abney . . . clearly use the term intensity to mean the energy of the light-waves coming to the eye" (p. 13). And again: "Abney was not at all concerned with the effect of the lightness or darkness aspect of the stimulus. . . . His purpose was to vary the energy of the light-waves coming to the eye by obstructing them by known amounts, and to ascertain the effect of this change upon the color limits. . . . Hence Baird is not justified in stating that Abney makes the brightness of the stimulus a factor in determining the color limit, etc." (p. 17). Dr. Rand's statement is in direct contradiction with Abney's own statement. Abney reports that he measured his stimuli not by a method which determined their physical energy in objective terms, but by a method which determined their brightness in purely retinal terms. (*Phil. Trans.*, Series A., 1898, 190, pp. 158 f.) His measurements of his stimuli were based exclusively upon subjective estimates of brightness; and he found that decrease in brightness of stimulus (through nine gradations) is invariably attended by a decrease in the extension of the

I cited as my authority. In my monograph (p. 20, footnote) I specified that the statement to which I referred is to be found on page 178 of Chodin's paper. Dr. Rand, however, selected her sentence from page 179 of this paper; yet she adds the erroneous statement that she selected it from the page which I cited (see Dr. Rand's paper, p. 53, footnote).

retinal zone (*loc. cit.*, p. 184). Dr. Rand states, however, in yet a third passage that "Abney and Landolt do not even claim that brightness difference affects the sensitiveness of the retina to color" (p. 52).

If any reader is interested in pursuing this matter farther, he will find that Dr. Rand has erred again in her references to the work of Landolt, Raehlmann, Klug and Bull; but it seems bootless to discuss these errors, or even to enumerate them in detail.

II. Several years ago Dr. G. M. Fernald reported that colored after-images may be obtained from stimuli whose colors are subliminal.[1] At the suggestion of Professor Titchener I repeated the experiments but my results were wholly negative.[2]

In a recent reference to these experiments, Dr. Ferree and Dr. Rand offered the following criticism: "Professor Baird reports negative results in every instance. With regard to this work the writers cannot help but observe that *Baird has failed to conform*[3] to the conditions which Fernald had said are essential for getting the phenomenon. Without drawing upon their own experiments for a knowledge of essential conditions, they will point out three conditions which *Baird has apparently failed to fulfil*,[3] the neglect of any one of which is amply sufficient to account for his results being negative. (1) Fernald lays great stress upon the use of a campimeter screen by means of the induction from which the brightness conditions were obtained which obscured the color in her stimulus. Baird used a simplified form of perimeter, how simplified Titchener and Pyle do not state. (2) Baird uses for the

[1] G. M. Fernald, 'The Effect of Brightness of Background on the Extent of the Color Fields and on the Color Tone in Peripheral Vision,' PSYCHOL. REV., 1905, **12**, 405; 'A Study of After-Images on the Peripheral Retina,' PSYCHOL. REV., 1907, **14**, 129 f.

[2] Instead of publishing my results I turned them over to Professor Titchener who was then interested in the more general aspects of this problem. (See E. B. Titchener and W. H., Pyle, 'On the After-Images of Subliminally Colored Stimuli,' *Proc. Amer. Philos. Soc.*, 1908, **47**, pp. 366–384.) Titchener and Pyle did not publish a detailed description of my experimental procedure, nor a complete statement of my experimental findings. They did, however, quote excerpts from my results, to which they appended the remark: "It does not seem necessary to publish the full set of results, though the data are at the disposal of any one who may wish to consult them" (p. 378).

[3] The italics of this anti-climax are mine. (See also the italicized phrase on the next page.)

duration of the stimulation intervals of 30 to 40 seconds. Fernald is careful to state that the interval of stimulation should not exceed three seconds. (3) In her description of conditions Fernald states that the color should be exposed behind the opening in a campimeter screen, and the card upon which the after-image is projected should be slipped in between the colored surface and the stimulus opening. Thereby the campimeter screen and thus the larger part of the field of vision remains unmoved and the least possible incentive is given for involuntary eye-movement. With Baird's apparatus, however, *we would judge*[1] that the ground upon which the after-image was projected must have been moved in between the stimulus and the observer's eye, thus exerting a strong incentive to drag the fixation with it."[2]

All three of these criticisms are based upon misapprehension of facts. If my critics had read my description of the apparatus and the procedure employed in these experiments[3] they would have learned (1) that the induction-screen in my apparatus was identical, in every essential particular, with that employed by Dr. Fernald; (2) that the duration of stimulation recommended by Dr. Fernald was employed in my experiment (when I failed to obtain positive results in exposures of two to three seconds' duration, I varied the duration of stimulation, in a few additional experiments, down to one second and up to one minute); (3) that the card, upon which our after-image was projected, invariably appeared behind the screen as Dr. Fernald recommended.

Since the Titchener-Pyle paper was published, Dr. Fernald has demonstrated to me that my failure to obtain positive results in these experiments was due to the absence of a sufficiently intensive light-adaptation during the preëxposure period; Dr. Fernald's demonstration has convinced me that the paradoxical after-image is a genuine phenomenon, and

[1] The italics of this anti-climax are mine. (See also the italicized phrases on the preceding page.)

[2] C. E. Ferree and G. Rand, 'Colored After-Image and Contrast Sensations from Stimuli in which No Color is Sensed,' PSYCHOL. REV., 1912, 19, pp. 198 f.

[3] I am informed by Professor Titchener that Dr. Ferree and Dr. Rand did not avail themselves of his offer to furnish these data.

that it is not a product of chromatic adaptation as I formerly believed. I gladly take advantage of this belated opportunity to call the attention of the reader to the profound significance of Dr. Fernald's findings; in my own opinion, they rank with the most important contributions made in recent years to the literature of color vision.